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ART. I.—*Observations on the epidemic Yellow Fever of Natchez, and of the South-west.* By JOHN W. MONETTE, M. D., of Washington, Mississippi.

(*Continued.*)

THEORY OF YELLOW FEVER MIASM.

To produce yellow fever as an epidemic in our cities, we conceived three circumstances or grades of action necessary:

1. *Miasm*, or the simple basis, which uncombined we think harmless.

2. The union of miasm with impure air, or with air which has been exhausted by respiration, and charged with human effluvia. This combination we term *malaria* or *infectious air*. This is the proper nidus or receptacle for the dissemination

and extension of personal infection; and is *always requisite* for the spread of this disease as an epidemic.

3. *Infection*, or the union of personal infection with *malaria* or infectious air. In this state the poison becomes active, and constitutes the efficient, predisposing, and exciting cause of epidemic yellow fever. It constitutes an *aerial poisonous ferment*, extremely subtile in its nature and properties.

These views we will endeavor to illustrate as we progress. The term *miasm*, as an agent in the production of yellow fever, we use in a sense somewhat different from its common acceptation, and, as we think, with equal propriety and reason.

I. *The Miasm of Yellow Fever.*—This we believe a subtile, gaseous, invisible and inodorous matter, generated by the action of the sun, or by solar heat, upon *common atmosphere*, independently of any effluvia, fetor, or exhalation from the decomposition of animal matters, or of any exhalation from marshes, dry earth, or vegito-animal compounds, or any of the commonly received sources of miasm. We believe it the result of some unknown combination of the solar rays with portions of the atmosphere, in certain situations, and under certain circumstances. These circumstances are, that the air shall be to a certain extent confined or stagnant; such as we have in cities in sultry oppressive weather, when the temperature in the shade is steadily above 90° of Fahrenheit during some portion of the day, or above 125° in a fair exposure to the open rays of the sun. The precise nature of this combination we are unable to explain satisfactorily. The doctrine of marsh-miasm conceives something generated by a similar process. We suppose this miasm to be perfectly free from morbific properties in its *simple state*, but that it becomes morbific by combination with impure air; that by its specific gravity it becomes particularly separated from common air, and settles near the surface of the ground, and in low, damp, confined places, similar to collections of carbonic acid gas; that it is entirely neutralised by a temperature of 45°, and is partially neutralised by a temperature as high as 55°; that it generates slowly, and only while the temperature is above 90° in the shade.

Hence we infer that it can accumulate only after several days of continued high temperature, during a period, and in situations where the extreme heat by day in the shade is above 90°, and the lowest temperature at night is above 55°. We suppose, judging from all the circumstances of weather, preceding all the epidemics of this kind in the United States, that it will not accumulate in prejudicial quantities, in less than 10 or 12 days of unremitted hot, calm, and sultry weather, such as we have described; that whatever amount may have accumulated in any number of successive hot days, will be destroyed or neutralised by one cold night, with the mercury at any time below 55°; or a strong wind, or a tornado, would disperse or waft it off. Hot weather favors its generation, and calm weather its accumulation.

We believe it requires a temperature of 90° in the shade for its generation, because the yellow fever has never spread as an epidemic in any place where the mercury of Fahrenheit by day had not ranged up to 90°, for at least 10 days previous to the epidemic, with a night temperature above 60° in the coldest exposure. We infer that one night, in which the mercury would sink to 55°, would destroy all that had accumulated in a week.

The *mean* temperature, as generally estimated, is no test—*no criterion*, but the *actual highest range of the mercury by day and by night.* We infer that the miasm once accumulated is not injured by damp, or foggy air, unless the temperature fall below 80° by day and 65° by night. If several days of damp, foggy, or misty weather, with high temperature, succeed a week of hot, calm, and sultry weather, the miasm previously generated remains in *statu quo.* Low damp situations in cities attract and retain it.

This miasm is seldom generated in our latitude, except in cities and towns, where the temperature in summer is always at least 3° or 4° above the temperature in the country; and this increased temperature is produced by the great amount of reflecting and radiating bodies in those places.

The miasm accumulates in the West Indies, more or less during at least six months in the year, because the temperature during that time is always above the point requisite for its neutralisation; and for more than nine months in the year, the temperature is not reduced to the point requisite to destroy or neutralize *infection*, or to 32°.

We infer this miasm is innocuous in a pure and often renewed atmosphere, because we find that in many cities and towns, when there has been free ventilation, there has been no epidemic yellow fever, although the mercury has ranged as high as 90° or 93° for several days together; but under the same temperature, with a stagnant atmosphere, constituting sultry weather, there has been yellow fever in its epidemic form. Stagnant atmosphere in a city becomes rapidly contaminated, where probably 100 or even 500 persons to every acre of surface are each incessantly respiring the air, besides the constant exhalations from their bodies. By these means alone, the mass of atmosphere in a city would become perfectly saturated with these exhalations in the space of a few days of sultry weather, besides being exhausted of its oxygen or vitalizing principle, whatever it be. The miasm uniting with this kind of atmosphere, becomes slightly morbific, and predisposes to yellow fever—*but not to bilious fever*, as we shall show hereafter.

In this state it becomes *infectious air*, or *malaria;* and this air, confined in a high temperature, and combined with the morbid effluvia from a patient laboring under yellow fever, after a few days acquires active morbific properties, which will produce malignant yellow fever in those who are unacclimated to such air.

When the air of a city is prepared for this morbific combination, the introduction of a few cases of yellow fever, or beds, and large quantities of porous and woollen goods from an infected part of the city, will greatly increase the danger of an epidemic yellow fever in such place.

II. *Malaria or infectious air.*—We have said that this union of miasm with the contaminated air of cities in hot sultry

weather, predisposes to yellow fever; and the longer the duration of such weather the stronger will be the predisposition, and *cæteris paribus*, stronger in *strangers* than in resident or acclimated persons.

The whole population of the city or town may labor under this predisposition, and yet remain healthy and unconscious of danger; and if nothing occur as *exciting causes*, they may escape an epidemic, or even sporadic cases. But on the contrary, some cases will be excited into action by extraordinary circumstances of fatigue, exposure, or by some imprudence.

Every case, *however excited into action*, sporadic or otherwise, increases the danger and the predisposition, in those immediately about the sick. At such times, the presence of a large number of *northern strangers* in the city is one of the most dangerous circumstances, and tends more than any thing else to lead on the epidemic: because the atmospheric predisposition, which would be innocent to the resident population, would excite sporadic cases in the strangers; and every sporadic case at such time increases the virulence of the local malaria, and thus hastens on the epidemic. Each patient with yellow fever at that time, throws off into the local atmosphere of his room, an effluvium, a subtile gaseous emanation from his body, which tends strongly to render that atmosphere morbific; and if that room be closed for several days afterwards, with a steady temperature between 85° and 90° of Fahrenheit, the air in it will become infected, and will produce yellow fever in some of those who may enter and breathe it only for a few minutes, and especially in such as are not acclimated. When the general atmosphere in the vicinity is becoming assimilated, strangers and unacclimated persons will be among the first, if not the very first, victims.

But, at such time of general predisposition, if steamboats, charged with infected air from an infected port, and freighted with goods and passengers from the infected district, arrive daily at the wharves, and discharge those goods and passengers who have already imbibed the disease; and are also visited by scores of the idle and curious, besides those whose

duty requires them to be on board, we shall soon see what are called sporadic cases springing up in different parts of the city, and chiefly, too, in those who *have been* in the vicinity of the wharves or *on board* the steamboats. Thus the epidemic yellow fever is always ushered in, not only at Natchez, but at all other points where it occurs on the lower Mississippi.

Hence, we conceive after miasm is formed, the *first stage* of contamination is produced in the general air of a city by the respiration, and the exhalations from human bodies in *health*; the *second stage*, by the emanations from the same laboring under this peculiar disease; and this constitutes *infection*, which is a virulent atmospheric poison.

III. *Infection.*—When the local atmosphere of a city, town, or even a house, has gone through all the previous stages, it possesses very virulent qualities, and assumes properties which are peculiar to it in this state. It then has become infection; and in addition to its morbific qualities, it has the property or power of extending itself in an impure atmosphere, or of assimilating to itself a certain portion of the contaminated atmosphere around. Portions of this infected air may be transported from one place to another, and then introduced into contaminated air, it will assimilate a circumscribed portion of it, until the latter will possess the same morbific properties as that which was introduced. This process we conceive to be a "gaseous ferment." Dr. Cartwright calls it the "assimilating process."*

We call it a gaseous ferment from the analogy which appears to exist between its operations and those of the common carbonic acid gas fermentation: like it this infection requires a certain basis for its action; it requires a certain degree of temperature for a certain time, with quietude. In both a certain process, the fermentative, will at length be completed; but when this process is complete, a small portion of the product, introduced into a similar basis, and in similar temperature, will excite the process of fermentation or assimilation much

*Medical Recorder, Vol. ix. pp. 9 and 10.

more speedily, and probably at a lower temperature than was required in the first process without the *leaven.* In all these particulars, as well as others, the analogy holds good.

Without the proper basis for the action of the assimilating process, the infection ceases to extend itself; thus it cannot extend the sphere of its virus without contaminated air, or through a pure atmosphere; for, as Dr. Paloni of Naples observes, relative to the yellow fever of Leghorn, which desolated that city in the autumn of 1804, "the infection of this fever is of such a constitution, that *pure and renewed air decomposes its fomes,* at a small distance from the sick; on the other hand, *air that stagnates, and is replete with animal exhalations, easily becomes a vehicle for it.* Hence it happens, that as soon as the disorder broke out, we saw it rage most fiercely in the *most filthy and least ventilated* parts of the city. *Pure, fresh, renewed air, destroys its infection.*"*

This is precisely what we contend for, viz: that yellow fever is *not infectious in a pure, free air,* and that it *is infectious or communicable in a contaminated atmosphere.*

We have already alluded to the diverse and opposite sources of yellow-fever miasm, as adduced by the advocates of its domestic origin in contradistinction to its introduction from infected ports or by infected vessels. There are scarcely any two ports or towns where it has been ascribed to the same combination of local causes. In every place where it has prevailed the source has been peculiar; no two epidemics have been produced by the same local causes, if the theories of gentlemen be correct. All the speculations and theories that have been adduced in favor of its local origin in any port or town in the United States, have had an advocate in Natchez or New Orleans; none having been too weak or too absurd to find a friend or admirer here.

We ascribe the epidemic yellow fever of Natchez and all our south-western towns, to one and the same cause, or to the same combination of circumstances, which are as follows:

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* Medical Repository, Vol. viii. pp. 426—7, &c.

1. Hot, dry, and sultry weather during August and September; but especially for 15 or 20 days in succession, immediately preceding the epidemic visitation; during which period the mercury of Fahrenheit shall range regularly as high as 90° by day, and above 70° at night, with that peculiar feeling of the atmosphere which is generally termed *oppressive*—or as Dr Cartwright designates it, a "breathless state of the atmosphere."*

2. The existence of yellow fever as an epidemic in New Orleans, at least 10 days before it appears in Natchez.

3. Regular, daily and uninterrupted steamboat communication with New Orleans.

4. A general state of salubrity in the city up to the outbreak of the disease, and the same state of salubrity through the country generally, both before and during the epidemic.

5. Public attention diverted to imaginary causes in the city, and apprehension quieted by a reliance upon the security gained by the removal of certain imaginary sources, and the prophylactic virtues of lime; while the real danger of importation is disregarded. These we believe to be the most invariable circumstances which precede epidemic yellow fever in Natchez; and Natchez was a stranger to these visitations previously to the autumn of 1817, which was soon after the introduction of steamboats on the Mississippi river.

As this was the first time that Yellow Fever was known as an epidemic in Natchez, I will give a short notice of it, for which I am indebted to Dr. Perlee, one of the most able physicians of the city for several years.†

Epidemic of 1817.—Occasional cases were seen in Natchez as early as August preceding the epidemic; but where they contracted the disease I have not been able to learn. The summer had been very hot, with occasional showers up to the first of September, with a sultry condition of the atmosphere afterwards. Dr. Perlee says, "the weather had been such as

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* See Medical Recorder, Vol. ix, &c.

† See Chapman's Medical Journal, Vol. iii. p. 6.

predisposed very strongly to violent disease, even early in September, and required only a small concentration of power to produce a sweeping epidemic." There had been no uncommon sickness, except this "strong predisposition to violent disease," which no doubt the Doctor thought he perceived. "At this time, when the population was *highly susceptible*, the Washington *steamboat* reached us from New Orleans, with persons on board, ill of yellow fever, some of whom were landed; and several young men from town went on board, who were all taken sick soon after and died. *The disease spread rapidly and with most destructive malignity.* For some time it had its sway over the whole city. On the 28th of September the physicians publicly announced the existence of yellow fever, and a large portion of the population retired to the country." It continued to prevail with slight interruptions until the 9th of November; and several cases occurred six days later in those who returned after frost to their houses which had been closed during their absence.

The whole number of deaths in the city, during this epidemic, was one hundred and thirty-four; besides some in the country who contracted their disease in the city.

The "predisposing causes" to which Dr. Perlee ascribed this epidemic, are the exhalations from stagnant pools, from city filth, from newly made ground in levelling the streets, and from an old grave-yard near the middle of the city. This heterogeneous mixture of effluvia, he supposed, produced the high state of "general predisposition," and prepared the inhabitants for an epidemic; and required only the aid of some *exciting* cause to produce a "sweeping epidemic." Although he was an advocate for the local origin of yellow fever, and would not ascribe it under any circumstances to importation, he leaves us to infer that the Washington steamboat with her yellow-fever patients, and her infected cabin, was the exciting cause which alone was wanting up to that time. In this we cordially agree with him, and believe that the same degree of predisposition, without similar "exciting causes," would every summer pass off without an epidemic. Although Dr.

Perlee was an advocate for the local origin of this disease, he evidently considered the arrival of the steamboat Washington as an important incident in the history of this epidemic. At that time not more than a half dozen steamboats were on the Mississippi, and the arrival of this boat from New Orleans was a matter of much interest and curiosity; so that many went on board to gratify their curiosity, and thus aided in introducing the disease into the city.

The epidemic of 1819 was the next visitation. It began on the 4th of September by a few scattering cases, and was not considered epidemic until the 14th of September. It continued to prevail with great mortality until the first of December, when 200 persons had fallen victims to its fury.

Dr. Perlee ascribes this epidemic to the atmospheric predisposition generated by similar causes to those enumerated in the last epidemic. At this time steamboat arrivals were so common that he does not refer to that as an exciting cause.

In 1819, the yellow fever raged with great violence in New Orleans, from and after the middle of August. Cases had occurred occasionally on board the shipping and about the wharves as early as June. Toward the middle of August it raged severely among the shipping and spread to the adjacent streets, and also to the boats which had descended the river. *Not a ship escaped* without the loss of one or two hands, and some lost their whole crews; many steamboats became infected, and lost many of their passengers and crews. It was not entirely destroyed until January following.*

During the years 1820, 1821, and 1822, Natchez was exempt from yellow fever epidemics; yet the writer is well convinced that during each of these years, and especially in 1822, there were several deaths from this disease in the city which were traced to infection from New Orleans introduced in bales of blankets and other woollen goods, and some cases in persons who visited New Orleans on business, and were attacked after their return.

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* Medical Repository, New Series, Vol. vi: pp. 6—20.

The epidemic of 1823.—This was probably the most terrific that has ever visited Natchez, or any other city of its population. The first cases occurred between the 12th and 20th of August. It continued to rage with great mortality until checked by frost near the first of November. By that time about 320 souls had died from this disease.

The weather for some weeks preceding this epidemic was very warm and oppressive. Dr. Cartwright says the weather in August presented each day, "several hours at a time attended with a breathless state of the atmosphere."

This summer yellow fever made its appearance quite early in New Orleans: many cases occurred among the ships' crews and others about the wharves as early as July; and it became epidemic about the first week in August. The arrival of steamboats from New Orleans was a thing of daily occurrence before the epidemic put a check to all kinds of business.

As soon as the disease was pronounced epidemic in Natchez this summer, as usual every family who had the means of escape, left the city. A few days afterwards, when the disease began to rage, ten or twelve families, including about sixty souls, removed to a cross-roads near Washington, known as Coonville, and there erected sheds and huts for a temporary residence. Being of the poorer class, they brought with them their beds, clothing, and all their movables, which were crowded into their small, ill-ventilated apartments. Here they remained in apparent security for a week or ten days, when cases of yellow fever began to develope among them. In a few days it spread among them with great mortality, and in a few weeks there were about twenty deaths among them, besides as many recoveries. The disease assumed its most malignant character here, and was precisely the same disease that prevailed in Natchez, and was equally fatal. At this time the whole country was as healthy as usual, and no disease of the kind existed any where, except where the infection could be traced to Natchez. However, at length the air of the encampment at Coonville became infected, and was avoided by persons in the vicinity as carefully as the air of Natchez itself.

But in the meantime five persons who had visited Coonville, and had been exposed to no other source of infection, contracted the disease and died in the country. It was admitted that the air of Coonville was as strongly infected as that of Natchez itself.

In this case the infected air of Natchez in the form of *fomites*, contained in beds, bedding, clothes, &c. was carried out and confined in these crowded, ill-ventilated wooden huts, until it took on the assimilating process, and produced an epidemic, which would have spread as readily through five thousand persons if they had been congregated there. No one ever presumed to ascribe this little epidemic to city filth, grave-yards, or putrid bacon.

Dr. Cartwright, in a lengthy essay published in the Medical Recorder, ascribed the epidemic in Natchez this year to a lot of putrescent bacon in the south-west part of the city, aided by other putrescent animal matters supposed to have existed about the city.* As to the bacon, I have long since been assured by Col. Fleming Wood, the alleged proprietor of the bacon, that the information on that subject was entirely erroneous; and that no such lot of bacon was in the city. Scarcely any one is sufficiently credulous, or so biassed by theory, as to believe that Natchez, which is known to be one of the most cleanly cities in the South, should be so infested with carrion, and other putrid matters, as are supposed to be necessary to bring on an epidemic. If any stranger were credulous enough to entertain such a belief, he would at once repudiate the idea when he should learn that Natchez has long been rather remarkable for the number of half-starved, rapacious vultures, which hover round in anxious expectation of such matters, which they devour in less than half the time required for the same performance by northern carrion-crows.

In 1824, the city was free from an epidemic.

Epidemic of 1825.—This epidemic began under the hill, or at the landing, among the clerks of a commission house and

* Medical Recorder, Vol. ix: pp. 5—7.

others near the steamboat landing. Cases began to multiply about the 20th of August. Several persons attacked were removed by their friends into the upper city, where they finally died. On the last of August the alarm was such, that a general flight of the citizens to the country ensued. The disease prevailed nearly two weeks near the landing before it began to spread in the upper city; and the first cases in the latter were easily traced to intercourse with the landing.

The disease continued to prevail until checked by a freeze on the first of November. The whole number of deaths in Natchez was about 150 souls. As usual, several deaths occurred late in November in persons who had left the city early, and returned after frost into their houses, which had remained closed.

This year the disease was carried to Washington. This is a pleasant, elevated, and cleanly village, six miles east of Natchez, containing about 100 hundred houses thinly distributed over a space half as large as Natchez. It had heretofore been the retreat of the merchants, mechanics, and others who wished to continue their trade with the country. This fall Washington was crowded with people, goods, and all the movables necessary for business and house-keeping. Nearly half the mercantile establishments carried their goods along, and every day for ten days brought out new supplies from Natchez and New Orleans. No unusual sickness prevailed in Washington or in the surrounding country. During the first twenty days of September, the mercury ranged as high 90° to 93° each day, and the air was sultry and oppressive.

During the first ten days after the arrival of the Natchez people, there were about 8 deaths from yellow fever in those who had contracted the disease before they left Natchez. About this time others who were citizens of Washington and who had not been in Natchez, began to sicken with yellow fever; and by the 18th of September the disease was considered epidemic in Washington, when nearly all the population fled.

The whole number of deaths in the Washington epidemic

was about 60 souls. About ten cases and five deaths occurred among those who returned into houses which had been closed, and were not entirely disinfected by the cold weather.

Previous to this summer, Washington was known as a healthy retreat to the people of Natchez during their epidemic visitations, where the disease had never been known to spread. No one pretended to ascribe this epidemic to filth, or the usual sources to which it is ascribed in cities. Washington is remarkable for the absence of every thing like city filth.

Dr. Cartwright, however, did ascribe it to a quantity of "putrescent bacon," which he alleges to have been carried out from Natchez. Even this would be an admission that the disease was carried from Natchez to Washington. But the fact is, there was no such "putrescent bacon" as that to which he refers. The writer was any eye-witness to the whole of this epidemic, and resided within one hundred yards of the alleged putrid bacon, and he does not hesitate to say that the Doctor was misinformed on that subject.

I hesitate not to declare it as my belief, that there would have been no epidemic in Washington, unless the people and goods from Natchez had been crowded into it. Besides it must be remembered the merchants received large supplies of fall goods from New Orleans while they were in Washington, and among these were 8 or 10 bales of blankets to meet the annual fall demand of the planters.

The first citizens of Washington attacked were my patients; and their infection could be traced clearly to opening and handling those bales of blankets, and other goods brought from New Orleans and Natchez. The disease was imported to Washington as surely as it was to Coonville in 1823.

The intercourse between Natchez and New Orleans was uninterrupted: goods were almost daily received at Washington on the same day they were landed at Natchez.

Dr. Cartwright ascribed the epidemic this year, to causes similar to those which he adduced in 1823; to which he added "spoiled porter," "rotten sour-crout," and also a boat with some corn in it sunk near the wharf.

Dr. Merrill ascribed it to "loose earth" exposed to the sun, in making and filling up a wharf, and in levelling the streets. Others ascribed it to such combination of circumstances or causes, as their fancy might suggest. These causes had no agency whatever in producing the epidemic; and we have shown already that such causes are harmless as generators of yellow fever miasm. We assert without fear of successful refutation, that whenever these circumstances have synchronised with a yellow fever epidemic, they were only incidental occurrences, and not in any manner essential to its existence. It is certain that several epidemics have appeared in Natchez since 1825, without either of the causes assigned by Dr. Cartwright or Dr. Merrill.

It has been urged against the doctrine of importation, that in 1823 and 1825, previous to the visitation of the epidemics, there was a regular quarantine against boats from New Orleans. I admit there was a mock quarantine, but none that was effectual. Each boat from New Orleans at that time was required to round to at the quarantine ground, and permit the health officer to inspect her, when she passed on up to the landing at Natchez. It is well known, that although boats were subjected to this formality, no one was ever prohibited from discharging either freight or passengers. There never was a quarantine at Natchez, *on proper principles*, until the summer of 1841, and that quarantine excluded the disease from Natchez, although it was carried on to Vicksburg. We will speak of this again.

The next epidemic visitation in Natchez was in 1829. This was the mildest epidemic ever known in the city. The city was deserted early and the disease did not spread extensively.

For several years the city of Natchez escaped the visitation of yellow fever until the fall of 1837, when it again made its appearance.

The epidemic of 1837.—This began with a few cases called sporadic, about the 8th and 10th of September; and by the 15th it was considered epidemic. Many of the physicians denied

the existence of yellow fever in the city until several cases terminated fatally with the genuine black-vomit, which none could dispute. The disease continued to spread gradually and with occasional abatements, until checked by frost about the 25th of November. The number of deaths from this epidemic was about 280, including hospital cases, many of which had been landed in a moribund state from steamboats direct from New Orleans. The epidemic this year was more mild and slow in its advances than usual, until the middle of October, when it began to rage with great malignity. This season the first cases in September frequently assumed some of the symptoms so mild that it was declared by some to be *bilious fever.*

The city of Natchez was as healthy as usual until the cases began to multiply; and it must be remembered that there were many cases of yellow fever landed from steamboats direct from New Orleans for ten days before any cases appeared among the residents of the city. The Natchez Hospital had been opened for the reception of indigent boatmen and others; and scarcely a boat passed up from New Orleans at this season of the year, that did not leave some yellow-fever patient at the hospital. It is a well-known fact on the lower Mississippi, that from the time yellow fever begins to occur in New Orleans, almost every boat that passes up leaves one or more yellow-fever patients at Natchez to be removed to the hospital, in passing to which they are carried through the most populous part of the city.

For several years previous to 1837 the Natchez Hospital had been closed against the reception of sick from the boats; and during this time there was no epidemic. But, a year before, the legislature had made provision for throwing open the hospital to the indigent sick; and it was in full operation when the yellow fever broke out in New Orleans, and scarcely a day passed without the reception of one or more patients from ascending boats, after the first of June; and after the first of August nearly all these were yellow-fever cases.

It may be worthy of notice here, that in the summer of 1841, when the quarantine was adopted, the hospital was closed, and no patients permitted to land from the boats, unless at private houses. As a general remark, there has never been yellow fever in Natchez unless when the hospital was open for the reception of indigent boatmen and others from the river. When the hospital is open, it is an inducement for all the steamboats passing up to land at Natchez, if for no other purpose than to relieve themselves of the sick, whom they might have to bury on their way above.

YELLOW FEVER IS NOT ENDEMIC IN THE UNITED STATES.

By this I mean to say, that *yellow fever is a specific disease*, having no perfect analogy, or any characteristic symptoms which identify it with bilious fever, which is endemic in the United States.

The circumstances under which these two forms of disease prevail are widely different; the persons liable to each are different; the times and places where each prevails are different; the symptoms and pathology of each are different; and the impression left upon the system after recovery, is different; a mild case of yellow fever is not a bilious fever; and a severe and malignant case of bilious fever is not yellow fever.

The following are some of the principal points of difference between the two diseases—viz:

1. *Bilious fever* prevails in the south, chiefly in hot and showery weather, and more commmonly in May, June and July; but sometimes in August and September. *Yellow fever* in the United States, never prevails as an epidemic until late in July, and generally, not before the middle of August or September; and always after a hot, dry, and sultry state of the weather.

2. *Bilious fever*, at times, prevails over certain districts of country, and generally among a sparse population, and is more common in country situations than in towns or cities. *Yellow fever* is confined as an epidemic, exclusively to towns

and cities, and appears *first* in commercial ports and large trading towns, and spreads only among a dense population, in an impure air.

3. When *bilious fever* prevails, it is caused by a *hot and moist* atmosphere, producing a degree of insalubrity extending over many miles of surface continuously, and without any circumscribed limits. *Yellow fever* prevails, or becomes epidemic in *hot and dry* seasons, and is restricted to certain limits, and to a local atmosphere as clearly defined as the bounds of a town or village, or the limits of one or more streets in a city; or one or more houses, or even to a single ship; while all the exterior air is perfectly salubrious, and the country population is uncommonly healthy, being often exempt from any general disease.

4. *Bilious fever* exists in all grades, from the simplest remittent to malignant bilious fever, terminating fatally, and yet preserves its distinct character, never assuming any of the pathognomic symptoms of yellow fever. *Yellow fever* in like manner exists in all grades from the mildest to the most malignant form, without losing its distinct and pathognomic character, whether it terminates in convalescence or death.

5. Persons who are natives of tropical climates, or those who have become acclimated to yellow-fever malaria, or who have recovered from one attack, are mostly exempt from a subsequent attack of yellow fever, but may have repeated attacks of bilious fever. Those who have been subjected to frequent, or annual attacks of *bilious fever*, are more liable to subsequent attacks, than those who have never suffered from it; and they are equally as obnoxious to epidemic yellow fever, as those who have never had bilious fever.

6. *Bilious fever* occurs in an atmosphere but slightly, if at all, contaminated by respiration; yellow fever, as an epidemic, depends for its existence upon air contaminated by human respiration.

Observant practitioners of the South, who have been familiar with yellow fever and also with bilious fever, do not pretend to identify the two forms of disease. They know by

experience and repeated observation, that they are essentially different forms of disease; that yellow fever is a disease *sui generis;* and that yellow fever and bilious fever do not prevail in the same local atmosphere. When bilious fever prevails generally in the surrounding country, the cities have no cause to apprehend epidemic yellow fever.

In this I am sustained by the testimony of all southern experience untrammelled by theory. The venerable Dr. Samuel Hogg, of Nashville, who practiced several years in Natchez, has assured me repeatedly of his firm conviction, that yellow fever is a specific disease, and partakes no more of the character of bilious fever, than of any other form of disease. In relation to the epidemic of 1837 in Natchez, he thus expresses himself in the Western Journal of Medicine and Surgery:*—"It was not, as has been supposed, a high or malignant grade of bilious fever; the whole surrounding country being more exempt from that disease than usual." . . . . "Also in most cases of recovery, the first satisfactory evidence of amendment, was a plentiful secretion of dark acrid bile."

After showing that it differed from all the ordinary diseases of the country, he remarks:—"If it were neither of the foregoing diseases, the question arises, *What was it?* I must briefly answer, that it was an assemblage of symptoms, or rational signs, indicating the modified condition of such organs as previous predisposing causes might have made obnoxious to the morbid influence of a specific cause, not cognizable to our senses—a poison *sui generis*, infecting a certain atmosphere."

On the 20th of October, 1839, the writer had an interview with the same distinguished individual, who had just returned from Nashville, by land, and through the interior of Mississippi, from north to south. The Doctor declared that in his route, he had passed leisurely through the whole length of the State, visiting most of the towns and villages on the way; and that while he daily heard of the ravages of yellow fever in

* See June No. for 1840—Hogg on the epidemic fevers of Natchez.

most of the cities and towns in the southwestern portion of the State, he was astonished to find so universal an exemption from disease of every kind, in the interior; and that he had not seen or heard of a case of bilious fever in the whole route. He then declared that the evidence was conclusive, that yellow fever and bilious fever were two separate and distinct diseases. He expressed his belief, that no discriminating observer, who had witnessed the facts of this summer, would contend otherwise; for the case presented by the summer and autumn of 1839, would amply convince any man of sound judgment. The writer has witnessed the same thing several times in the lapse of twenty years. From the experience of twenty years' practice in the vicinity of Natchez, I have learnt that a hot and dry season during the summer months is a certain guaranty of health; a hot and wet season is a sure concomitant of sickness, and especially of bilious and congestive fevers. As a general rule it is known proverbially to be healthy in the country while yellow fever is raging in the cities and river towns. The inference is conclusive, that ordinary bilious and remittent fevers originate from causes, and under circumstances, entirely different from those which lead on yellow fever in the cities. So far as analogical reasoning can convince, these facts prove clearly that yellow fever is a disease *sui generis;* besides which we see that the symptoms, the accession, the course, the duration, and the termination, are all different in the two diseases. The organs and tissues mainly implicated are different.

Yellow fever is properly a disease of tropical climates. Like some tropical plants it may be transported to a more northern climate, and for a time may flourish; but the frost of our winters will utterly destroy it, and it must be annually renewed. Without this annual transplanting or renewal, in our climate it would die and become extinct. But the constant commercial intercourse with the tropical ports, serves to keep up the annual supply of this exotic. It can be successfully transplanted in our climate, only for a short time, when the weather in certain sections of the country is strongly

assimilated to the climate of the West Indies—such as in the island of Curacoa.

To establish the diversity between yellow fever and bilious fever, I am extremely happy to be able to cite the authority of Professor Caldwell,* one of the most distinguished teachers of medicine in the United States. He supposes the poison of yellow fever may be constituted of different proportions of the same elementary principles, which enter into the morbific miasm of bilious fever; but that the proportions of these constituents are so varied, that the poison resulting is entirely different in its effects upon the human system. He remarks, that, "wherever genuine yellow fever reigns as an epidemic, it reigns alone." Speaking of the peculiar nature of yellow fever, he continues: "Throughout the whole range its type is the same. It continues to be every where yellow fever; although in different places of very different grades. Under its lightest and simplest modification it is less severe, and more easily cured, than intermitting fever. Yet in type and character it is as different from that disease now, as when marked by its highest grade of malignity. Its strength and power to injure are gone; but in its form it has suffered no mutation."

In illustration of the diversity of the diseases, I will state the following fact which fell under my notice. A friend of mine had fifteen negro slaves in New Orleans, where they had become thoroughly acclimated to yellow fever infection, and were exempt from its attacks during several epidemics in the city. In the spring of 1835, he removed them to a plantation on the Mississippi river a few miles above Natchez, and about 300 miles above New Orleans. During the following summer bilious fever prevailed to a considerable extent through the country, the weather being hot and showery. During the season every one of these negroes had severe attacks of bilious fever, in its severest grade, and one of them died from it who had been raised in New Orleans, and was not liable to yellow

* Medical and Physiological Memoirs—Prize Questions, pp. 210—211, &c.

fever. The others were removed to New Orleans in 1838, and remained there during the most malignant epidemic of 1839, in the most perfect health. This principle might be illustrated in a hundred cases. But it appears to me unnecessary to enter further into a discussion of the question whether yellow fever be a disease *sui generis*, or only a high grade of bilious fever.

I have been told by enlightened physicians and teachers of medicine in the region of the Ohio, that the two diseases are identical; that the bilious fevers on the Ohio frequently run into the yellow-fever grade; and that they have seen genuine yellow fever in the Ohio valley, of local origin. I do not hesitate to declare it as my opinion, that they are altogether mistaken on that point. A very strong argument in my favor is the fact, that scarcely a year passes without many young physicians emigrating to the south, as fully imbued with those doctrines as their learned teachers could desire; and they never do recognize yellow fever when they first meet with it; and almost invariably it is a source of much mortification to them afterwards. The older practitioners who are familiar with it, recognize it at once, and in any stage; those who have never seen it, but who are confident of their judgment, almost invariably deny the existence of yellow fever until it has made havoc among their patients and derided their skill. The medical faculty of Vicksburg in 1841, denied the existence of yellow fever in their city for two weeks after it was sweeping them off at the rate of 8 or 10 a day. This was the case too after several of them, unconsciously, had seen cases in the fall of 1839. In both cases it was called congestive fever. Our most eminent physicians, on their first introduction to the disease, were placed in the same unpleasant predicament. An intelligent physician of Vicksburg, informed me in December 1841, that for two weeks they had no suspicion of yellow fever, and considered the epidemic as some new form of congestive fever; and of course the treatment was not such as it would have been under a certain knowledge of its true character.

Along with the few advantages which have resulted from the dissemination of the doctrines of Dr. Rush, relative to the local origin of yellow fever, it must be admitted that several important evils have come. Of these, not the least is the utter disregard of many prudential measures, calculated to prevent the spread of the pestilence, as well as its introduction into our sea-ports. His doctrines inculcate an imprudent exposure of the healthy to the sources of disease, to infected atmosphere; and thereby to unnecessarily endanger the lives of our citizens, and the prosperity of our most flourishing cities. This exposure is inculcated under a vague conception of the terms, contagion and infection, at least, by those who are thus induced to become its victims. Relying upon some indefinite meaning of the terms, and utterly ignorant of the true laws which govern the origin and spread of yellow fever in the south-west, many enterprising and generous young men from the north, have been annually sacrificed to the prejudices of education. Hundreds every year fall victims, who. under proper views of this disease, might pursue the path of safety.

(TO BE CONCLUDED.)

ART. II.—*On the Impropriety of Mercurial Salivation.* By Dr. THOMAS TOWNSEND, of Wheeling, Virginia.

A salivation from mercury, though not generally dangerous, is so great an inconvenience, and so much dreaded by patients, as frequently to deprive the physician of this valuable article of medicine. It is not at all surprising that patients dread a mercurial sore mouth, for in truth it is a loathsome, disgusting, and painful disease; and frequently worse than the disease for the cure of which it was produced. This being the case, as all candid medical men must acknowledge, the question presents itself, can mercury be used, and its agency as a remedy be obtained, without the inconvenience of a mercurial sore

mouth? With the three exceptions of *acidity of the stomach, diseased* and *spongy gums*, and where the medicine cannot be made to *operate in due time*, I answer in the affirmative. If then, every medical man may obtain all the remedial agency and beneficial effects of mercury without its inconvenience; and if the very object of the profession is, not to inflict pain, but to relieve it, he is bound by every principle of uprightness, as well as humanity, not to add to the afflictions of his patients, the suffering and pain of a disgusting, loathsome, mercurial sore-mouth. If this be true, why salivate at all? what agency has salivation in the cure of disease? why then give mercurial preparations with a view to produce salivation? This is an important inquiry, and should receive the candid investigation of the medical profession. If ptyalism has not a decided physiological agency in the cure of disease, it certainly should not intentionally be produced.

All medicines act on the living system by stimulation. Every medicine has a specific action of its own, differing in some one or more particulars from the operation of every other medicine, on account of the peculiarity of its stimulation, or the particular part or branch of the system, upon which its impress is made. And this is as true of the preparations of mercury as of any other medicine. The most obvious specific effect of mercury is that of stimulating the salivary glands into such an increased action as either to amount to actual inflammation, or to a state of things bordering upon it; and attended with inflammation and ulceration about the mouths of their ducts. I will here make a quotation from the United States Dispensatory of Wood and Bache, for the purpose of showing what these gentlemen consider the modus operandi of mercury, and what no doubt is the generally received opinion of the medical professsion on that point, and more particularly what is considered *its constitutional effect.*

"Of the modus operandi of mercury, we can say nothing farther than that it seems to act through the medium of the circulation, and that it possesses a peculiar alterative power over the vital functions, which enables it in many cases to sub-

vert diseased actions by substituting its own in their place. This alterative power is sometimes exerted, without being attended with any other vital phenomenon than the removal of the disease, while at other times it is attended with certain obvious effects, indicative of the agency of a potent stimulant. In the latter case, its operation is marked by a quickened circulation, with a frequent and jerking pulse; by an increase of activity imparted to the secretory functions, particularly those of the salivary glands and the liver; by an exaltation of the nervous sensibility; and, in short, a general excitement of the organic actions of the system. When its effects are not otherwise obvious than in the subversion of disease, its operation may be presumed to be the same as when it produces obvious stimulating effects, though so slight and imperceptible as altogether to escape notice.

"When mercury acts insensibly as an alterative, there is not the least disturbance of the circulation; but when it operates decidedly and obviously, it is very prone to let the brunt of its action fall upon the salivary glands, causing in many instances, an immoderate flow of saliva, and constituting the condition of things denominated ptyalism or salivation. Under these circumstances, to the alterative effects of the mineral, are added those of depletion and revulsion. Occasionally its depletory action is exhibited in an increased secretion of urine, or an immoderate flow of bile; and where ptyalism can not be induced, and either of these secretions becomes considerably augmented, the circumstance ought to be held equally conclusive of the constitutional effects as if the mouth had been affected."

I suppose from this quotation that the ptyalism is taken and held to be the obvious evidence of the constitutional effects of mercury. But during its administration, if an increased secretion of urine, or an immoderate flow of bile take place, we ought to hold this "equally conclusive of the constitutional effects as if the mouth had been affected." And "when its effects are no otherwise obvious than in the suppression of disease," and "its operation presumed to be the same," I

would ask why we should not "hold this equally conclusive of its constitutional effects as if the mouth had been affected." As it is so "very prone to let the brunt of its action fall upon the salivary glands," causing either "an immoderate flow of saliva," or an ulceration of the insides of the cheeks, and of the neighboring gums, and soreness of the teeth, &c., "constituting the condition of things denominated ptyalism or salivation," may we not with great propriety call this the *local specific effect* of mercury? In other words, does not salivation furnish the obvious evidence that the medicine has produced its local specific effect?

The specific effect of this article is salivation. If mercury be determined to the mouth it will expend its whole force about the mouth and salivary glands. Every practitioner of much experience knows, that if he have a patient with a mercurial sore mouth, and in this condition of things he give a dose of calomel as a cathartic, it will not operate, but will go to the mouth and make it sorer; and he will be compelled to prescribe some other cathartic. The ptyalism is so great an inconvenience to the patient that, if possible, the remedial agency of mercury should, at all times, be obtained by what Wood and Bache call its "alterative power," which is more properly "its constitutional effects" than ptyalism, the action of which is, at first, entirely local and confined to the vessels about the mouth. But when the inflammation of the mouths of the salivary ducts, and of the gums, and the periosteum of the fangs of the teeth, becomes considerable, the irritation from this local inflammation produces sympathetic fever, for the same reason, and on the same physiological principle as the local irritation and pain from the inflammation of wounds, such, for instance, as amputation, or from other local inflammations followed by sympathetic fever. This inflammation and sympathetic fever, although attended by "a quickened circulation and with a jerking pulse," are not sufficient, in my estimation, to establish the opinion that this is "the constitutional effect" of mercury, but rather its specific effect. If the term "constitutional effect" be applicable at all to the effects of

mercury in its modus operandi, it should rather be applied to the effects which it produces in other parts or branches of the system, than the salivary glands. Thus it will be seen that, instead of adopting the commonly received opinion, that a mercurial salivation is the evidence of the constitutional effects of the mineral, I consider this circumstance the evidence, that it has ceased to be a general or constitutional remedy, and has become a local irritant. For so soon as the medicine begins to be determined to the mouth, all that may be given will go the same way, and keep the mouth sore so long as the medicine may be continued. If we wish the agency and remedial power of this medicine to operate on other portions of the system, the local or specific effect should be avoided. The question then very naturally occurs, can this be done? I answer it can.

In the early part of my practice I entertained the common opinion, that salivation ought to be frequently produced, not only in syphilis and fevers, but in chronic diseases. In two cases of fever occurring nearly at the same time, I prescribed calomel, and directed the patients to drink as much cold water as they wished, and told them that if the mouth became sore so much the better. I visited them daily, and they both recovered without being salivated. These cases led me to make observations for myself, the result of which was, that cold water for common drink, did not, and would not, determine calomel to the mouth, and make it salivate, according to the generally received opinion. At a later period, from having so frequently observed a salivation take place when acid drinks, such as tamarind water, lemonade, or cream of tartar, were used during the administration of calomel, I came to the conclusion that acids must have something to do with the production of this effect. I then began to experiment, until I became fully satisfied, that the use of acid drinks during the administration of calomel, and the salivation, stood to each other in the relation of cause and effect. After becoming satisfied of this fact, I forbade acids altogether, during the use of calomel; and since that time I have had no case of salivation.

when patients would rigidly follow directions. After a few years' observation and experience of this mode of using and managing mercury, I became fully satisfied of the superiority of obtaining its remedial agency and alterative effects without the troublesome inconvenience of a salivation.

I have no hesitation in saying that a cure is more speedily and better performed by calomel, or any other mercurial preparation, without ptyalism than with it. As mercury operates through the medium of the sanguiferous system, there is no doubt in my mind that it operates by stimulating into increased action both the secernent and absorbent vessels of the system. By a judicious combination with other articles, which operate on the secernent or absorbent system, mercury may be determined to the one or the other, and in this way act as an alterative, so as to change diseased action, and restore the system to a healthy condition, much better than when permitted to go to the mouth and produce salivation. Colchicum and sanguinaria, in certain doses, act as alteratives, and change diseased action, without any material evacuation, either by vomiting, purging, or sweating; and such, I apprehend, is the operation of mercury when, as is said in the Dispensatory, "it subverts diseased action without any obvious effect" on the circulation. But it is usually followed by evacuations from the intestinal canal. To this effect of operating on the alimentary canal, after having made its peculiar impression on other parts of the system to which it had been directed, may be attributed the superiority of mercury as an alterative over almost every other article in the materia medica. In fact, if it be prevented from being determined to the mouth, it may be made to operate on the capillary arteries, veins, and absorbents, and the whole glandular system.

I consider its operation on the salivary glands as definitely specific as that of the specific operation of secale cornutum on the muscular fibres of the uterus. If permitted to go to the salivary glands, it will expend the whole force of its stimulating power upon those parts as certainly as the latter medicine exerts its action upon the

uterus. And, as the associated action of the abdominal and other muscles results from the increased activity of those of the uterus, so the increased activity of the vessels about the mouth may bring into association, through the medium of nervous influence, an increased activity of the whole sanguiferous system, as manifested by the "frequent jerking pulse."

During the last twelve or fourteen years, I have had no case of salivation in my practice, except such as may be considered accidental—that is, where patients, contrary to directions while taking mercury have used acids, or where the stomach was loaded with acid at the commencement of using it, or where the medicine could not be made to operate so as to be carried out of the system in due time, or where the gums were diseased, spongy, and inflamed, and the action already determined to the mouth. The first of these accidental circumstances depends entirely on the patient and his nurses. The second may, in general, be easily remedied by giving before the administration of calomel, a sufficient quantity of bicarbonate of soda to correct the existing acid, and by giving a little from time to time to neutralise that which may continue to be generated, until the medicine shall have operated and been carried out of the system. I know no remedy for the diseased and spongy gums, but to go to a dentist, and get the gums cured and the mouth put in order before the individual becomes otherwise sick! If the patient has neglected this, when he falls sick, and mercury becomes necessary, he must run the risk of a sore mouth, and submit, when it takes place, with the best grace he can. With regard to the slow and constipated state of the bowels, which retains the article too long in the system, or until it becomes determined to the mouth—when we have reason to suspect sore mouth, this can only be prevented by using other cathartics more speedy in their operation, and assisting them by enemata. I know no other circumstances, when acids are strictly avoided, that will determine calomel to the mouth, and make it salivate, but the three just enumerated.

I adopted the practice of so administering and managing calomel as to avoid salivation, under the full conviction that its action when determined to the glands about the mouth, is local, and not general over the constitution—that when it produces ptyalism, it acts as a local irritant, and that if we wish "its constitutional effects," or rather its operation on other parts of the system than the salivary glands, it must be so managed as to prevent its determination to the mouth.

The view I entertain, that salivation is not the constitutional, but the local specific effect of mercury, has been greatly strengthened from finding every medical man, with whom I have conversed on this subject, and who advocated the old doctrine, unable to give a rational physiological reason why a patient laboring under bilious remittent fever, for instance, should be salivated; or why salivation should be resorted to in a case of enlargement or chronic inflammation of the liver or spleen; or why this effect should be produced for the cure of incipient cataract, or removal of opacity of the cornea from abscess upon that membrane. Can any one explain the modus operandi, so to speak, of salivation, in the cure of any one of these diseases—unless by referring it to the doctrine of sympathy?—a term which, Dr. Porter says, "will cloak ignorance under the appearance of science, as well as any other," and cut off all farther inquiry and investigation, so as to be passed off as an explanation of what is not understood. I can not believe, then, that ptyalism is the constitutional effect of mercury, until a clear, rational, and physiological explanation can be given why the local inflammation about the mouth is constitutional. Until this can be done, I must be permitted to consider this local inflammation as the local specific effect; and that it is only in the absence, or rather avoidance of this local effect, that we are able to avail ourselves in full of the general remedial agency of this medicine in other parts in which we wish it to operate, or to which we wish its agency directed.

I hope the reader will not hastily condemn these views because they may be new to him, but will try them by the

test of experience, and if he finds them to be true, have candor and magnanimity enough to acknowledge their correctness. But if on a full and fair trial by experiment he finds them untrue, then I shall expect him to give a rational physiological explanation of the modus operandi of mercury, such as will show conclusively their fallacy. I have been long intending to publish these views, but felt fearful, because they may not be considered orthodox. But when I reflect that the profession to which I belong cannot be considered as having arrived at that state of perfection, at which investigation should cease, because the profession is incapable of farther improvement; and when I reflect further, that a physician who makes no attempt to improve his profession, lives to but little purpose, I have at last ventured to submit these views, with the hope that if they do nothing more, they will induce those whose talents, acquirements, and station so much better fit them for the purpose, to test by experience my modified method of managing and using this potent, useful, and necessary article of the materia medica—an article indeed, for which, in many cases, we have no substitute. And besides this, and as a further reason for desiring others to try this method of employing mercury, I may remark, that one of the professed objects of the medical profession is, to relieve the distress, and pain, and suffering of our fellow beings. This being the case, humanity requires, not only that we should do this to the utmost of our ability, but that in doing it, we should not give any additional pain, or by the use of our remedial agents, produce any increase of suffering that can be avoided.

As I feel assured that all the beneficial agency may be had from the operation of mercury, as an alterative or cathartic, without the inconvenience of a mercurial sore mouth; and, as I believe, further, that the general agency and efficacy are more certain and more efficient, more agreeable to the patient, and more consistent with the principles of humanity, I hope to live to see the day when no enlightened, high-minded and honorable physician will designedly produce a

mercurial salivation; but will unite, as he ought to do, in removing any prejudice in the minds of the people, not only against this drug, but against the medical profession for its employment.

Jan. 13, 1842.

ART. II.—*Uterine Hemorrhage, with expulsion of the Ovum, successfully treated.* By Dr. J. H. HOLMES, of Spring Ridge, Mississippi.

I was called on the 13th day of August 1839, in consultation, to see Mrs. H., aged 30, of sanguine temperament, and in the third month of pregnancy. I learned that she had received a fall from a horse in the early part of the day, one or two hours after which she complained of severe pain in the back and loins, occasionally extending to the uterus; these pains continued for some time, accompanied by a profuse discharge of blood from the vagina.

The midwife who had been called to see Mrs. H., becoming alarmed at her situation, had Dr. F. sent for, who, on his arrival, immediately bled the patient, gave her an anodyne, and ordered cold applications to the abdomen. These means for a time arrested the discharge; finally it returned with more violence than ever, and he ordered a pill of the acetate of lead and opium every hour. An examination was now made to learn the situation of the os tincae; this was found considerably relaxed and dilated.

At 3 o'clock, P. M., the flooding was violent, accompanied by trifling pain, frequent syncope, and a very small, quick pulse. At the suggestion of Dr. F., I was sent for from a distance of twelve miles. When I arrived, at 9 o'clock P. M., I could find no pulse at the wrist; the flooding continued slowly, with complete atony of the uterus; there was frequent syncope and vomiting, and occasionally delirium. I inquired of Dr. F. and the midwife, if the ovum had been expelled;

they were confident that it had not come away. I was not disposed to make an examination myself in regard to the situation of the os tincae, from the fact that frequent touching is not only injurious by fatiguing the patient, but by removing the coagula that may be important in stopping the hemorrhage.

I advised the free administration of ergot in decoction. We had some prepared in a few minutes and given to her, but the stomach instantly rejected it. Alkalies and absorbents were then resorted to, and a sinapism was applied over the stomach with a view of quieting it. The ergot was again tried with a hope that it would be retained; but it seemed that we were only to meet with disappointment on all sides.

I suggested the propriety of making Mrs. H.'s situation known to her husband, and urging a speedy delivery; this was done by Dr. F., he being the family physician. The reply was, that he would rather see her die than that we should make an effort of that kind. The ergot, acetate of lead, opium, and cold applications having failed, we had but one alternative left, that of using the tampon, and leaving nature to effect a delivery. However, in the midst of this dilemma, we proposed to Mrs. H. to allow us to rupture the membranes and thus bring about a speedy delivery; to this she readily consented.

I was selected to make the examination, and see what could be done. Introducing the index finger of the right hand, I found the os externum very much relaxed, and the os tincae considerably dilated; upon extending the finger beyond the os tincae, I discovered that the placenta was loose and flabby, but still adherent to the fundus and right side of the uterus. I also convinced myself that the ovum had been discharged at some time during the early part of the day. I endeavored to get hold of the placenta between the finger and thumb, and extract it, but having failed in this we procured a small wire, bent at one end in the shape of a hook. The curved extremity of the wire was conducted along the finger of the right hand to the fundus of the uterus, and being drawn down gently, was hooked into the placenta, which it brought away. A tampon of old

linen previously oiled was introduced with a view of arresting any further hemorrhage (for at this moment every ounce of blood was of immense value to the patient), until a contraction of the womb came on. Her complete exhaustion, cold extremities, and frequent syncope, induced us to apply a roller of flannel, dipped in warm turpentine, to her extremities. In two hours a partial reaction had ensued, and by morning we had the pleasure of seeing our patient so far improved as to consider her in a fair way to recover with care. The tampon was extracted next evening; her recovery was very slow and gradual, being confined to her room for four weeks.

I have been induced to report this case, not for any thing novel in its treatment or management, but for its rare occurrence, and the success that attended our imperfect wire crotchet.

September, 1841.

Selections from American and Foreign Journals.

Action of Spirits upon Habitual Drunkards. By Prof. C. H. Schultz, of Berlin. There are two points into which this subject naturally divides itself:—1*st*. What is the nature of the action of ardent spirits on habitual drunkards? 2*d*, Why does not wine produce, equally with spirits, those morbid effects which are observed in the habitual drunkard?

Professor Schultz distinguishes between intoxication—a merely temporary effect of ardent spirits, the result of their physiological and medical action in excess—and their pathological action, which shows itself in its highest degree in the production of delirium tremens. Intoxication is a brief excitement, and one, to a certain extent, normal, which ceases when the remote cause is removed; but tremors and delirium are the result of a morbid reaction, which continues for a long time after the cause has ceased to act. In investigating the nature of the morbid condition to which habitual spirit drinking gives rise, we must take no account of its short stimulating effect, which is rather the opposite of the disease produced by spirits, than its first stage.

Some writers have regarded this disease as an over-excitement of the nervous system, amounting almost to an inflammatory condition, but ending in exhaustion. Others have considered it to be the result of the direct action of spirits on the blood, and have regarded the nervous symptoms merely as secondary. The dark color of the blood of drunkards, which several persons have noticed, favors the supposition, that some special change is produced in that fluid. The causes, however, by which the color of the blood may be modified are so various, that a mere knowledge of the fact that such a change occurs, is not enough to solve any of the difficulties of the subject. The supposition that this change consists in an excess of hydrogen and carbon in the blood, is contradicted

by the dissimilarity of the symptoms which intoxication produces, from those which result from respiring any of the narcotic gases. The analogy between the states of the blood under those two conditions is merely apparent, and has not been shown to exist in any point besides color. Orfila and Renard have attached much importance to the chemical action of spirits on the mucous membrane of the intestines; alcohol coagulating albumen, but having in other respects an antiseptic property. It has been supposed, by the means of this property, to interfere with digestion; but this opinon is in many respects erroneous, for there is a great tendency to decomposition in the contents of the intestinal canal of habitual spirit drinkers.

Dr. Schultz is of opinion, that the primary action of ardent spirits is upon the organs of vegetative life. Two points connected with the subject have especially attracted his attention:—1*st*, The influence of ardent spirits on the bile, and its consequences. 2*d*, Their influence on the coloring matter of the blood, and on the envelope of the blood corpuscles.

If alcohol is added to bile, and the solution evaporated for a short time, the bile loses its alkaline reaction; it likewise ceases to be precipitated by vinegar, dilute sulphuric or muriatic acids, or by solutions of oxalic or phosphoric acids. The sour contents of the stomach of rabbits, dogs, and oxen, throw down no precipitate from the bile of oxen, when mixed with alcohol, and a long time is requisite for neutralizing the acid, while sometimes that change does not take place at all, if the bile and accohol have been long mixed together. This fact throws a new light on the disordered digestion of drunkards, and especially upon the generation of acid in the stomach, and the sour eructations to which they are so especially liable. But the bile is not only directly subservient to digestion, for it contains besides a number of excrementitious matters. In the healthy state these matters are got rid of by being precipitated in insoluble flocculi, which are then voided with the feces. The alcoholic solution of bile is not precipitated by acids, or by the contents of the stomach, or but very imperfectly, and consequently these excrementitious matters are retained in a state of solution in the intestinal canal, and become mixed with the chyle. The occurrence of jaundice, and many of the icteric symptoms to which drunkards are liable, may be partly explained, by supposing some of this morbid bile to become absorbed, and to enter the circulation.

It has long been known that a great part of the ardent spirits taken into the stomach, is absorbed unchanged, and that

alcohol thus finds its way into the blood. Hitherto, however, persons have contented themselves with saying, that the blood has more of the characters of venous blood than natural, and they have not inquired which of the constituents of the fluid undergoes the morbid change. Professor Schultz has made the following experiments:—If a small quantity of spirits of wine is added to fresh blood, the fluid becomes transparent, and its natural color changes to a cherry red, but it does not become blacker, as is commonly asserted. If the blood is now examined under the microscope, it will be seen that the coloring matter generally is changing its situation, and, instead of being contained in the blood corpuscles, becomes gradually diffused through the plasina. Thus, (if the alcohol is added to blood deprived of its fibrine,) in the course of a short time, instead of colored corpuscles floating in a colorless plasina, the red plasina will be seen to contain colorless corpuscles. If the spirits are added to fresh blood capable of coagulating, the red plasina will form into a gelatiniform substance of the consistence of thick milk, and no separation into crassamentum and serum will afterwards take place. If half the quantity of spirit is added to the blood, coagulation of the plasina, or of the albumen of the serum, in cases where the blood has been deprived of its serum, at once takes place, and forms a mass of the consistence of cheese. These alterations are produced by injecting spirit into the veins of living animals, as well as by adding it to blood out of the body, and instant death of the animal takes place, although the change of the blood does not extend beyond that part of the venous system into which the injection is thrown. The changes in the capsule of the blood globules likewise merit attention. They lose their color gradually, and contract more or less in size according to the quantity of alcohol added. In some instances the blood corpuscles contract almost to a point, and become nearly undistinguishable, so that the blood appears to be a uniformly-transparent red fluid, without any globules. The newly-formed corpuscles undergo these changes most rapidly, while the alterations take place much more slowly in the larger globules, which contain more coloring matter.

The changes which the blood undergoes, are not directly chemical, for alcohol does not dissolve the coloring matter, but the corpuscles seem to lose it owing to their contraction. The constituents of the blood, then, are not actually decomposed, as was formerly imagined; but the results of the action of alcohol are not on that account less pernicious. The process of restoration is dependent on the normal condition of

the blood corpuscles, since they are the media through which the mutual changes in the air and blood are effected. The state of the coloring matter is also a point of great importance, as is evident from the striking changes which the air produces, and which are so marked that we look on the alteration of color as the chief point of difference between venous and arterial blood. But both the contractility of the blood globules and their contents, are greatly modified by the action of spirits on the blood. Thus we arrive at an explanation of the alterations in the respiratory process of the habitual spirit drinkers, which result principally from the proper changes not taking place in the blood corpuscles and their contents. Less oxygen than natural is absorbed, hence less carbonic acid is exhaled, and the blood assumes a venous character, though one widely differing from that which the narcotic gases produce, since they produce a dilatation of the blood globules, and an accumulation of coloring matter within them.

From these alterations Professor Schultz deduces the various morbid conditions of the nutritive process incidental to spirit drinkers. The action of spirit on the nervous system is usually physiological rather than pathological. Delirium tremens indeed is the result of a pathological process; not however of exhaustion consequent on over-irritation of the brain and nerves, but rather of a destruction of their excitability, owing to the morbid changes in the blood.—*Monthly Jour. Med. Sci.* Oct. 1841, from *Hufeland's Journal*, Apr. 1841.

Absence of the Uterus. By Dr. Bertani.—Caroline Fossati, twenty years of age, when on the point of marriage, came to the hospital at Nular, to be treated for amenorrhœa. It was then ascertained that she had never menstruated, nor ever had had bloody discharge from any part of her body, which could supply the place of that secretion. Upon examination, the labia and nymphæ were found to be well formed. The clitoris was somewhat small. The urethra was in its natural situation, and of its ordinary shape. Below it, the caruncule were in the form of an ellipsis, and appeared to surround the orifice of the vagina. On separating them, the mucous membrane was seen to be continuous, and not dilatable, scarcely yielding when a sound was pushed against it. Upon introducing a finger into the rectum, and an instrument into the bladder, they were found to be separated by no great thickness of parts. On examining all sides of the bladder

with the sound, and depressing with the finger the abdominal parieties, no trace of a uterus or vagina could be detected.

The carriage and voice of Fossati were somewhat masculine. Her breasts began to appear at thirteen. There was hair on the pubes at fourteen; and at fifteen she had some abdominal pain in the loins and hypogastrium, which returned every month, but never with any discharge. Upon being told that she could not marry, she appeared not to suffer any grief. She had no sexual appetite. In other respects her tastes and desires were feminine.—*Jour. de Med. et de Chirurg. Prat.*, Nov. 1841, from *Annali Univers. di Med.* 1841.

Rotatory Movements of the Yolk in the Ovum of Mammalia, during its passage through the Fallopian tube.—The attention of Prof. Bischoff of Heidelberg was called to this phenomenon by an observation of Dr. Martin Barry in the Philosopical Transactions for 1839. Dr. Barry there mentions having on one occasion met with an elliptical vesicle (filled with a transparent fluid in which were small elliptical granules), adherent to the mucous membrane of the Fallopian tube. In the centre of this vesicle was a mulberry-like body which continued rotating itself for half an hour, the rotation subsiding by degrees into a tremulous motion. Professor Bischoff was of opinion that this vesicle was an ovum and the rotating body its yolk, and he adduces an observation in corroboration of this suppposition.

Having carefully laid open, with a pair of scissors, the Fallopian tubes of a rabbit, which had been placed with a male during the previous eight days, he found four ova close together about the middle of the tube of the left side. Within the zona pellucida of these ova was the yolk which, however, did not completely occupy its area, but between the yolk and the inner surface of the zona a transparent fluid intervened. In this fluid each yolk ball continually rotated on its own axis, in the direction from the uterus to the ovary. On examining the ova with a power of 800 diameters, the surface of the yolk was seen to be furnished with very minute ciliæ, by the vibration of which this movement was effected. Professor Bischoff convinced himself, by very careful examination, that the ova themselves remained perfectly still during the continuance of this rotatory motion of the yolk, which ceased on the preparation being moistened, in order to prevent the ova from drying up.

In this phenomenon Professor Bischoff sees a fresh point of coincidence between the processes of developement in mammalia and in other animals, since similar rotatory movements of the yolk have been observed in the ova of mollusca and polyps, and recently by Von Siebold and Ehrenberg in those of medusa aurita.—*L. and E. Monthly Jour. Med. Sci.*, Oct. 1841, from *Muller's Archives*, Heft. 1, 1841.

Composition of False Membranes.—M. ANDRAL has clearly shown that the quantity of fibrin in the blood is increased during inflammation, and that the proportions of this substance bear a close relation to the different degrees and stages of the inflammatory affection with which it is connected. Some recent researches on the composition of false membranes, by M. LASSAIGNE, throw additional light on this interesting subject.

I have already (says Mr. Lassaigne) shown that the false membranes which are thrown out on mucous membranes in a high state of inflammation, are not composed of coagulated albumen, as many anatomists have supposed, but formed chiefly of a large proportion of fibrin, mixed with some soluble albumen, and moistened by a yellow colored serum, containing all the organic and inorganic elements of the blood. I have, more recently, examined the false membrane in a pig laboring under pseudo-membranous angina, and obtained the same results. The false membranes, obtained from this source, were white with a yellowish tinge, slightly elastic and extensible. On submitting them to pressure, I obtained a viscid yellowish fluid, which turned test-paper blue, and coagulated under the influence of heat and mineral acids.

A portion of the false membrane was frequently washed in cold water, to remove all the soluble matter; the residuum was a white substance, which presented all the physical and chemical characters of fibrin extracted from the blood. When digested with weak acetic acid, it became swollen, then transparent, and was entirely dissolved on the application of gentle heat. The solution, when saturated with caustic potassa, threw down white flocci, which were again dissolved by an excess of the alkali. Concentrated sulphuric, nitric, muriatic acids, and the solution of the ferro-cyanuret of potassium, threw down a white precipitate, as they do with the acetic solution of fibrin.

The water in which the false membranes were washed,

was now evaporated, and there remained flocci of coagulated albumen; on continuing the evaporation to dryness, there remained a saline residuum, composed of chloride of sodium, carbonate and lactate of soda, and a little phosphate of soda, salts which exist in solution in the serum of the blood.

From these facts we may conclude, 1st. That the false membranes thrown out by serous and mucous membranes in a state of inflammation, are principally composed of the fibrin of the blood. 2. That this principle, being separated from the circulating fluid, together with a small quantity of albumen, becomes organized, and thus gives rise to the morbid products alluded to.—*Prov. Med. Jour.*, from *Journal de Chimie, June* 1840.

Evolution of Tubercle.—Dr. C. Haller has had ample opportunity of observing the various forms in which tubercles in the lungs are evolved, in the House of Correction, Vienna, where phthisis is extremely common, "where *lung-tuberculosis* is endemic," as the author says. Here, as everywhere else, the most common of all the forms of tubercular development in the lungs, is, 1st, that of a *bronchial catarrh*, or seemingly simple cough, with or without febrile symptoms, and accompanied either with very little or no expectoration; 2d, often the disease commenced in the guise of *pneumonia;* 3d, still more frequently it assumed the appearance of a common continued fever; and, 4th, in rare instances it presented itself with the perfect tertian or quotidian intermittent type.

The first form is sufficiently well known—description has been exhausted in regard to it. But the form in which pneumonia is simulated, has attracted less attention. Here violent fever, with great dyspnœa, or rather breathlessness, a sensation as if a load lay upon the breast, without much or any positive pain referred to a particular spot, distressing and almost dry cough attacking in fits, violent action of the heart, with an accelerated, hard, and often irregular pulse, a burning skin, great sense of depression, and rapid sinking—all the symptoms, in short, indicating acute pneumonia; but this diagnosis is not borne out by the information conveyed by auscultation and percussion. The chest sounds well everywhere; it is even tympanitic in its tone. The respiratory murmur is appreciable at every part, only somewhat rough, with occasional small, but, by degrees, increasing, mucous rattle; in a word, auscultation and percussion lead only to negative results.

The symptoms continue for several days unabated either by blood-letting or the treatment generally followed under the same circumstances, and, so far as symptoms are concerned, with success; though, if the exudation of tubercular matter be extensive, the patient may be lost within from a week to a fortnight. If the tubercular exudation have been limited to the upper part of the lungs, the symptoms are less severe, and there is less immediate and apparent danger; the dyspnœa becomes less by degrees, the cough becomes moister, but the febrile state continues long, and, when it ends, it is without a crisis of any kind. The little relief procured by blood-letting, the great amount of dyspnœa, the slight indications of inflammation presented by the blood that is abstracted, the negative results of auscultation, the pertinacity of the fever, and the want of critical phenomena, particularly by urine, suffice to distinguish this disease from true pneumonia.

From proper typhus fever the diagnosis of tubercular formation is more difficult. But the disease in this case is always preceded for a long time by a greater or less amount of indisposition. The patient feels an extraordinary degree of languor, and has an extremely irritable and variable pulse; the heart and vascular system are greatly excited without any apparently sufficient reason, inasmuch as no organ in especial seems to suffer. Nervous symptoms are, at the same time, manifested: confusion of thought, rambling talk in the night, dry tongue, burning thirst, hot dry skin, trembling of the extremities.—the symptoms, in a word, are those that usher in an attack of common continued fever; but they do not increase in intensity, and after about a fortnight, the patient begins to hack and cough; the nervous symptoms abate, and now it becomes obvious that he is laboring under a serious affection of the chest, which, in the event of the tubercular exudation, which is its element, having been extensive, carries him off in the course of a few weeks in what is called a *galloping consumption.*

Dr. Haller also observed the primary formation of tubercle to take place upon several occasions, along with symptoms of the well-marked intermittent febrile type. But as the qualities of air and soil that give diseases an intermitting character are wanting in this country, we shall not follow the writer here.

With his excellent opportunities of observing incipient tuberculosis, Dr. Haller is enabled from certain signs to perceive the disease before it appears, and when it is only threatened. These signs are mental and corporeal. Those who are about to become the victims of tuberculation of the lung, are

either greatly depressed and miserable, or they show a singular irritability and changeableness of disposition. The sleep is generally disturbed by distressing dreams. The subject of observation loses his healthy look and gets thin; sometimes the appetite is increased, sometimes it is greatly diminished; the digestion is imperfect, the bowels are moved more frequently, and the evacuations are larger than usual. Frequently the most remarkable symptoms are referable to disturbance of the circulation; there are symptoms of determination to the head, vertigo, headache; now and then epistaxis, and the individual is subject to change of color constantly; there are symptoms of congestion about the chest, a passing sense of oppression, some cough, perhaps slight hæmoptysis, though this occurs for the most part at a subsequent period; palpitation readily excited, and so obstinate as often to cause suspicions of organic diseases of the heart; indeed, it is only by the use of the stethoscope that the true state of affairs can be discovered.—*Lond. and Edin. Monthly Jour. of Sci.*, Nov. 1841, from *Med. Jahrb. des Œsterreich, Staates*, July, 1841.

M. Raciborski on the Physiology of Menstruation.—Our readers will remember that in our recent review of Gendrin's *Traite Philosophique*, we drew their attention to this curious and hitherto ill-understood subject of physiological inquiry. From the researches of this gentleman, and from those of M. Negrier, as well as of Dr. Robert Lee, it was suggested that there is an actual rupture of one of the ovarian vesicles at each period of menstruation, and that the sanguineous discharge from the uterus was the result of this lesion. M. Raciborski questions the accuracy of this statement. While he admits that the primary movement in each act of menstruation is a congestion of the vessels of the ovaries, he denies that any rupture of their surface necessarily takes place at the same time.

M. Raciborski sums up the conclusions to which he has come, after a very elaborate inquiry, in the following propositions:—

1. That menstruation is a consequence of the accomplishment of the development of the ovaries.

2. That it is the direct result of the means employed by nature to place the ends of the Fallopian tubes and the ovaries in the relations necessary to fecundation and the passage of the fecundated ova.

3. That the sanguineous congestion, which is indispensable for obtaining those conditions in the human being, appears sufficient in itself to explain the occurrence of the hæmorrhage which constitutes menstruation—without having recourse to supposing that there is any necessary solution of continuity.

4. That the vertical position, favoring still more the effects of sanguineous congestion on the generative organs, may be one of the principal reasons of the abundance of the menstrual flux in women, and in some species of simiæ.

5. That, for want of having precise information as to the nature and theory of menstruation, it has been hitherto impossible to establish a rational treatment of the various disorders induced by irregularities of this function.

6. That it is not yet sufficiently proved that the ovula arrive successively to maturity at each menstrual epoch, or that the most mature ovum then approaches nearest the surface of the ovarium, there to become ruptured and give exit to a germ.—*Med. Chir. Rev. Oct.* 1841, *from L'Experience.*

Physiology of Conception.—We find the following interesting case in the November number of the *Archives Generales de Medicine*, where it is credited to Riecke's Prussian Medical Gazette.

A girl aged twenty eight, servant in the family of a peasant, had for some time been suspected of a criminal intimacy with her master—her mistress at length surprised them in *flagrante delicto* and turned the girl away—two days after this the dead body of the unfortunate girl was found in a neighboring pond, an autopsy was ordered by the magistrate, and the organs of generation were found in the following condition:

The labia majora were a little tumified, the hymen very much stretched and recently torn, much mucus in the vagina. The bladder, rectum, and other pelvic organs, were very much injected, the uterus was of a deep red color and more than one third larger than in the normal state. The cavity of the organ which was also increased in size was filled with a sanguinolent mucus, of which that portion immediately in contact with the walls of the uterus was most consistent. The lining membrane was red and soft. The right fallopian tube was red, and its size about three times greater than in the normal condition; it was swollen in the middle. A pretty large

sound (*un sonde assez epaisse*) penetrated the canal with ease and encountered no obstacle till it reached the uterine extremity of the tube where the swelling existed; the cavity of the tube was filled with a white fluid. The right ovary was double the size of the other, it was embraced by the fimbriæ of the fallopian tube and appeared very much injected.

We observed upon its surface two chinks of the size of a lentil, and very red at the bottom. From each of these issued a delicate membrane half an inch long, and bearing at its extremity a small ovule (petite œuf) about the size of a grain of mustard seed. Each of these ovules presented, even to the naked eye, upon their surfaces, a net work of vessels very distinct indeed; they contained a transparent fluid. The ovary having been laid open, we found within it a red coagulum granular towards the centre; this coagulum communicated with the chinks before mentioned.

From these facts the presumption was, that conception took place two days before, but it was impossible to arrive at any thing like certainty.

Remarks.—This case is one of the most valuable yet recorded in the illustration of the physiology of conception. It is exceedingly to be regretted that no microscopic examination of the ovule was made; some of the vexed questions of obstetrics might possibly have been settled, had these minute ovules been subjected to accurate examination—particularly as to whether the ovule has two coats, as obstetric writers generally have taught, or three as Von Baer asserts, or only one as some of the late physiologists pretend, the chorion, according to their view, being a capsule added to the ovum during its transit through the Fallopian tube. The excessive vascularly observed in this case in all the pelvic organs and especially in the uterus is very interesting; how well it illustrates the remark of Breschet, that the formative process, like the reparative, partakes largely of the character of inflammation. The state of the sanguinolent mucus by which the uterus was occupied, fluid in the centre and becoming of more solid consistence upon the uterine wall, shows us very distinctly how the decidua is formed, and why it happens that this shut sac when fully developed should contain a fluid in its cavity. The sanguinolent mucus is separated into two parts, a soft solid immediately applied upon the uterine wall and there forming the decidua (perione of Breschet,) and a fluid, the hydro-perione of Breschet, occupying the cavity of the membrane.—*N. Y. Med. Gaz.*

Diagnosis of Inflammation of the grey, and the white matter of the Brain.—Prof. Bellengeri gives the following diagnostics to enable us to decide what part of the brain is inflamed. The white matter is the organ of motion, the grey of sensation and intellect—hence when the grey matter is irritated or inflamed, the intellectual faculties are disturbed with loquacity, and delirium; the senses, especially those of touch, are dull. Cerebral congestion in consequence of the compression it produces upon this substance, produces numbness and somnolency. When the white matter is affected, there is a lesion of the motive apparatus, spasms or paralysis, especially if the optic thalami, the corpora striata, or the cerebellum is attacked.

If the disease is not intense there is spasm, but if it is, then paralysis occurs. Spasms of the flexor muscles (*emprosthotonos*), indicates an affection of the cerebral hemispheres, spasm of the extensors (*opisthotonos*), one of the cerebellum. When the senses and the motor apparatus are alike affected, we should conclude that the lesion extended to both the white and grey matter. Diseases of the spinal marrow easily affect the motion because the white matter abounds there. To prove this fact the Professor relies upon the cases of Royer, Collard, Bayle, Rullier, &c. He believes, contrary to the opinion of Bell, that the posterior columns regulate motion, the anterior sensation. Simple pain in the head, particularly when severe, and not accompanied by delirium, torpor or spasm, indicates a lesion confined to the membranes, the dura mater, pia mater or arachnoid; encephalitis, accompanied by delirium, indicates that inflammation is seated in the grey matter of the brain, especially on the periphery of the organ. It is true that in encephalitis with delirium, whether acute or chronic, autopsy often reveals inflammation of the membranes, but the author believes that the delirium should not be attributed to the affection of the membranes, but to its being propagated to the grey matter of the substance of the brain, which is contiguous to the membranes.

As all the superficies whether external or internal, is covered with a pia mater, we cannot conceive of inflammation limited to the grey or the white matter, and not propagated to the membrane from which they derive their vessels. There cannot, therefore, be there a phlogosis of the cerebral substance without meningitis. In comatose or apoplectic encephalitis, the disease whether phlogosis or congestion, attacks the deeper seated portions of the grey matter; if it follow a phlogosis, its intensity indicates the severity of the disease

and the need of further depletion, unless it depend on serous effusion which can be recognized by symptoms, and requires appropriate treatment. Convulsive, spasmodic or paralytic encephalitis, indicates an affection of the medullary (white) matter.—*N. Y. Med. Gaz. from the Jour. des Sciences Med. de Turin.*

Clinical Remarks by Dr. MARSHALL HALL, *on the Use of Setons.*—Many years ago, I was consulted by Mr. Doubleday, of Blackfriars Road, in the case of a young married lady who had suffered from peritonitis after her first accouchment.

This peritonitis appeared to be confined to the pelvic region. Its acute character had been subdued, but tenderness, with tumidity, and difficulty in voiding the bladder and rectum remained. I made a careful examination. A distinct hardness was felt under the pubes, extending to one side, I think the left. On examination per vaginam and per rectum, a similar hardness was found occupying the lower part of the pelvis. I imagined this hardness to consist in coagulable lymph, effused from the inflamed peritoneal surfaces of the pelvis, producing the symptoms by its pressure on the neck of the bladder and on the rectum.

We strictly regulated the diet and the intestines, and inserted an ample seton over the induration. Slowly and gradually that induration, with its attendant symptoms, became diminished, and eventually disappeared.

Several years after this, I was consulted in the case of the sister-in-law of this patient, under very nearly similar circumstances. The same remedy was followed by the same happy result.

A year ago I was consulted by Mr. Burford, in the case of a gentleman of sixty, who had become affected with pain, tenderness, and tumidity of the abdomen. On a careful examination, a distinct hardness was felt in the midst of the general tumidity, occupying the region of the caput cœcum coli. We regulated the diet and the bowels, administered mercury, and inserted an ample seton. The mouth became affected, and the seton discharged copiously: the hardness and the other symptoms gradually, but at length entirely disappeared

A similar case occurred a year ago, in the person of a gentleman of forty, a patient of Mr. Squibb, in Orchard street. A strict regimen was enjoined, the bowels regulated, and an

ample seton was inserted. The induration, which in this case occupied the space between the false ribs and the ilium, on the left side, gradually disappeared.

Two years ago I was consulted by Mr. P———, a barrister, affected with pneumonia of the middle and upper lobes of the right lung. A seton was inserted, and Mr. P——— went to Madeira. On his return, the physical signs and the symptoms of pneumonia had disappeared.

I have still more recently treated a case of pneumonia of the upper part of the right lung, in consultation with Mr. Beane of Peckham. A seton was inserted, and in six weeks a most decided amendment in the physical signs, the symptoms, and the general health occurred. Since that period the patient has continued to improve, and now no dullness on percussion, or other signs of disease, is perceptible.

In a variety of cases of acute or chronic, local or limited internal inflammation, I have had recourse to the seton, and uniformly with the most marked success; so that, I think, we may look upon the remedy as almost specific in such cases. It is unnecessary to enumerate them. But hepatitis and nephritis belong to them in an especial manner, and I would suggest this remedy as likely to be of service (if any remedy can) in the case of albuminous urine. In one such case the urine was more albuminous after cupping. I imagined the effect arose from the mechanical violence inflicted, and recommended the cupping to be performed above and below the precise region of the kidneys. Under the use of this remedy the albumen diminished, and even ceased for a time.

These and other cases, then, induce me to think that there can be little doubt of the real efficacy of the seton in chronic inflammation. The object is to demonstrate this in some measure, and then to notice briefly farther applications of the remedy. I do not pretend to suggest any thing new, but rather to enforce what is old. The efficacy of setons, when appropriately applied in the nucha (for they are frequently employed very uselessly), is well known. The proper cases are inflammation and congestion. But the case to which I would particularly draw attention is that of disease of the spinal marrow, with paraplegia, or paraplegic spasm.

In this case issues are generally inserted. They appear to me far more painful, far less manageable, and far less efficacious than ample setons. They have also, I am persuaded, been generally applied below the real seat of the disease. I was consulted a few weeks ago by a gentleman from Manchester. With partial loss of power, he had loss of sensation

in the lower extremities; the numbness extended to a line just above the sacrum. Issues had been applied on each side of this line. They might, with equal efficacy, have been applied to the foot! I need not say that the spinal nerves proceed, for some distance, from above directly, rather than obliquely, downwards, and that the seat of the disease is *at* or ABOVE their junction (insertion or origin) with the spinal marrow.

Bearing these two principles in mind, then, viz. that ample setons afford a more efficacious counter irritation than issues, and that they ought to be applied higher along the spinal column than has been usual, I think we have a new mode of treatment for this formidable class of diseases.

These setons should, besides, be larger than usual. They should be three-fourths of an inch in breadth, and extend through two inches in length, be inserted on the level with and above the supposed seat of the disease (the anatomy being consulted), and be four or six in number, two or three being instituted on each side of the spinal column. Acting on this principle, I had, five days ago, the pleasure to receive the most satifactory account of a patient affected with paraplegia whom I had seen at Lohan, in Essex, in consultation with Mr. Gross.

I repeat, and beg to conclude by repeating, that I believe counter irritation applied along the spine has failed, because it has been applied below the seat of the disease; and that, to be efficacious, it must be both more efficient in itself, and applied with greater regard to the anatomy of the spinal marrow and nerves. The precise spot for their application must be left to the well-informed practitioner. I need scarcely remind my reader, that the persistence or cessation of all reflex actions, will determine whether our remedies should be applied above or below the origin of the cauda equina, above or below the last dorsal vertebra.

We are great advocates for setons too. We confess we have not used them so broad nor so many at a time as our friend Dr. Hall. But perhaps he is right. The difficulty would be, in some cases, to induce patients to submit to their introduction.

We have tried setons in three or four cases of albuminous urine. They were bad cases certainly, and the effect was not encouraging.—*London Medico-Chirurgical Review*, from *London and Edinburgh Journal of Med. Science, No.* 11.

Reduction of Luxations of the Thigh.—The Journal of the Medical Society of Montpelier, contains a memoir on a new method of of reducing luxations of the thigh. This method was first proposed by M. Collin in an inaugural thesis presented to the Faculty of Medicine at Montpelier in 1832, and is now advocated by M. Jaumes, of the same place, who gives some very striking cases. We content ourselves with giving a sketch of the plan; its simplicity commends it to a trial, at least, and if it be found as useful in other hands as in those of M. Collin, he will certainly have conferred a benefit upon science.

It is first necessary to put the patient on a plank strong enough to sustain the weight of the body; the width should be about two feet, the length somewhat to exceed that of the trunk; in this plank four holes are to be bored near the four corners, through which cords are to be passed so as to suspend it about four feet from the ground; the cords, at one end, to be six inches shorter than those at the other, so that one end of the plank shall be six inches higher than the other. This plank being covered with a thin, firm mattrass or other like bedding, the patient is to be placed upon it. flat on his belly, the arms to hang down on either side, the pelvis situated at the highest end of the plank, rests upon the anterior superior illiac spines, the lower extremities hang over the end of the plank so as to form an angle slightly acute with the body. The patient should be allowed to remain in this position a short time without any efforts being made by way of traction on the dislocated limb; the position alone, merely by the weight of the limb, in one of M. Collins cases, restored the dislocated bone. If this does not happen, slight traction may be made *directly downwards*; if this fail, weights may be attached to the limb, a towel being tied round the ankle, and forming a bag beneath the foot, into which weights may be placed. Such is the simple apparatus and the mode of using it, by which M. Collin asserts that he has reduced several cases; one that had resisted extension and counter extension for hours. The plan will, of course, only apply to luxations backwards and upwards on the dorsum of the ileum. M. Jaumes enters into a long argument to prove the reasonableness of this plan; this we shall not translate. The plan has been tried and succeeded; it is simple and painless; let it be tried again,—*N. Y. Med. Gazette.*

Reduction of a Prolapsus Uteri after sixteen years' continuance.—M. DURANT records an interesting case of this in the Transactions of the Medical Society of Ghent. The womb protruding beyond the external parts, and covered by the inverted vagina, presented a globular tumor, round, and contracted at its origin into the form of a circular appendix. The *os uteri* was clear at its inferior part. The tumor at its middle part was fifteen and a half inches in circumference. Its external surface was brownish red, and covered with crusts and ulcerations. The long continuance of the affection had seriously injured the general health of the patient—she was pale and emaciated, and subject to sleeplessness, and cramps of the stomach.

Mr. Durant, before attempting reduction, kept the patient on light diet, and at rest in bed in a proper position; at the same time dressing the tumor with opiated emollient fomentations. Its surface speedily softened, and the crusts fell off, leaving superficial sores. After six days of this treatment the operation was performed. It having been ascertained that the rectum and bladder were empty, the patient was placed in the position most advantageous for the entrance of the womb. M. D. then introduced the right forefinger into the os uteri, and burying it, pushed upwards in the axis of the tumor which itself was placed in the axis of the true pelvis—then retaining the uterus in its place with the left hand, withdrew the finger, and, repeating this manipulation with gentleness, just as one turns the finger of a glove outside in, accomplished the reduction in less than half an hour. He then inserted into the vagina a sponge, cut into the form of a cylinder, and saturated with an emollient decoction, the thick end being highest up, and a cord attached to the other, for the purpose of removing it at pleasure. This sponge-pessary was retained in its place by means of compresses, and the T bandage. The patient did well, speedily gaining flesh and strength. During the after-treatment, which continued for about six weeks, emollient and astringent lotions were employed, and an ordinary-sized caoutchouc ring-shaped pessary was used, the saturated sponge and the injections being passed through its centre.—*Am. Jour. Med. Sci.*, from *Journ. de Med. et de Chirurg. Prat.* Feb. 1841.

Union of Fractured Bones.—The following cases which we extract from a paper published by A. Thierry, in L'Experience, Nov. 4, 1841, are curious and also practically important, as illustrating the dependence of the reparative pro-

cess on the state of general health. The fact that the existence of a venereal taint in the system will so seriously interfere with the formation of a callus, and the consolidation of the fractured bone, is new to us.

Case 1. M. aged 13, clerk in a wine store, fractured both bones of the fore-arm in descending into the vaults. The apparatus of Boyer was applied, and at the end of sixty days removed, the union being supposed to be perfect. On examination the consolidation was not complete, yet this young man enjoyed good health, and there was nothing to explain why the fracture did not unite. The patient was closely watched and it was soon discovered that the girl who nursed him, afforded him from time to time a physical solace for his ennui; she was discharged, the apparatus re-applied for sixty days and the consolidation was complete.

Case 2. A school boy of 14 had the arm broken by the fall of a bench. The displacement was inconsiderable, the fracture was reduced and put in the old apparatus; on removing the dresssings, the bone had not united. The boy was watched and it was discovered that he was addicted to masturbation. The apparatus was re-applied and means taken to prevent his recurring to his old habit. The cure was complete.

Case 3. M. D. making a false step in the street, fell and fractured the thigh above the great trochanter. The fracture was reduced and the apparatus of Boyer applied. At the end of sixty days, on removing the apparatus, the bone had not united. The patient was asked whether he had not some venereal affection; on his replying in the affirmative an antivenereal course was pursued, the apparatus was re-applied, and in twenty four days the fracture was consolidated.—*N. Y. Med. Gaz.*

Ergot.—Dr. Wright inclines to the opinion that the formation of this excrescence is owing to the combined influence of atmospheric heat and moisture. He is confirmed in this opinion by ergot being much more common in the district of Soloyre—a moist, rich soil, hot and sheltered—than elsewhere. Grains also occur, only one half of which is ergotted, an insuperable objection to the notion that ergot is produced by the growth of a fungus, which would of course attack all parts of the grain. Besides, from his own experiments, he finds that ergot in powder or grains, sowed with rye, fails to produce the disease in the growing plant. Wa-

tering the growing plants freely, with water in which ergot had been steeped, failed in like manner; so did the application of ergot to the growing ears of rye. The excrescence remarked at the upper extremity of the grain, he regards as the *stigmata* altered by disease.

Dr. Wright's conclusions were further confirmed by discovering that ergot contains a considerable quantity (26 per cent.) of fecula, which it would not do if the disease was produced by fungus. Dr. Wright has the merit of discovering fecula in ergot—no previous analyst makes mention of it.—*N. Y. Med. Gaz. from Ed. Med. & Surg. Jour.*

Feigned Hysteria.—Some of the shapes assumed by this pathological proteus are hideous and disgusting. Paralysis of the muscular fibres of the bladder, or spasm of its sphincters, sometimes really occurs; sometimes it is only aped, in hysteria. It is a common trick with these patients to pretend that they labor under *retention of urine*, and that, although the bladder is full, they cannot make water. The daily introduction of the catheter by a dresser or apprentice, appears to gratify their morbid or prurient feelings. Sometimes, no doubt, the difficulty is real, but it is oftener feigned or exaggerated. I have again and again known it to disappear, upon the patient's being left, without pity, to her own resources. But girls have been known to drink their urine, in order to conceal the fact of their having been obliged and able to void it. The state of mind evinced by many of these hysterical young persons, is such as to entitle them to our deepest commiseration. The deceptive appearances displayed in the bodily functions and feelings find their counterpart in the mental. The patients are deceitful, perverse and obstinate, practising, or attempting to practice, the most aimless and unnatural impositions. They will produce fragments of common gravel, and assert that these were voided with the urine, or they will secrete cinders and stones in the vagina, and pretend to be suffering under some calculous disease. A young woman contrived, in one of our hospitals, to make the surgeons believe that she had *stone in the bladder*, and she actually submitted to be placed on the operating table, and to be tied up in the posture for lithotomy, before a theatre full of students, and then the imposture was detected. Sometimes they simulate *suppression of urine*, and after swallowing what they have passed, vomit it up

again, to induce the belief that the secretion has taken place through a new and unnatural channel.

It is impossible, I say, not to pity the unhappy victims of this wretched disorder, when their morbid propensities drive them to such acts as these. I mention them, because you must expect to meet with such cases, and because, while you take care not to express your suspicions prematurely, or on light evidence, you should be on your guard against the mortification of being deceived, by the false signals held out, into active and ill directed measures of treatment.—*Dr. Watson's Lectures on the Practice of Medicine*, in *Lond. Med. Gazette*, June 11, 1841.

Poisoning by the Vapors of Antimony.—M. Lohmeier, of Schonebeck, has recorded in the Nos. of *Casper's Wochenschrift* for April and May 1840, the history of four patients who were frequently exposed to the vapors of antimony, in an establishment where there were prepared on the large scale tartrate of antimony, butter and glass of antimony, and other antimonial preparations, during the preparation of which were disengaged abundant vapors of antimonious and antimonic acid, and chloride of antimony.

The four patients presented the following symptoms. Pain in the head; lancinating pain along the edge of the ribs and in the back; difficult respiration with mucous and sibilous rattles over the chest; difficult expectoration of tenacious mucus; sleeplessness, anorexia, diarrhœa, profuse perspirations, and general weakness; dysuria with a mucous discharge, causing a burning feeling in the urethra; flaccidity of the penis, with loss of the sexual appetite, and even complete impotence; pain in the testicles, and atrophy of these organs, as well as of the penis; pustules over different parts of the body, but especially on the thighs and scrotum.

M. Lohmeier observes, that in none of the recorded cases of poisoning by antimony, has the peculiar affection of the medicines on the sexual organs been discovered, nor yet its effect in exciting a peculiar cutaneous eruption; but it is remarked that the homœopathists have noticed this on the generative organs, and that its preparations are habitually administered to animals by agriculturalists to hasten their fattening, when it probably acts by diminishing the sexual appetite; and lastly, that it appears to have been used by monks in the monasteries with the view of diminishing their sexual propensities, and hence its name of antimony, from *moine*, a monk.

The curative means used by Mr. Lohmeier were the free use of antiphlogistics in the first place, and afterwards the internal administration of opium, tannin, and quinine, with lotions of the same applied to the parts externally affected, and milk diet. If the functions of the sexual organs did not return naturally as the other affections abated, he recommended the use of tincture of cantharides united with opiates, and lotions of cold water to the scrotum.—*B. and F. Med. Rev.*, Oct. 1840, and *Edin. Med. and Surg. Journ.* Jan. 1841.

Beneficial Effects of Narcotism in some obstinate cases of Neuralgia.—M. Levrat read to the Academy of Medicine at their meeting of the 8th of June last, a memoir on this subject. From the results of his long practice, it appears that opium given to the extent of producing narcotism, often cures cases of neuralgia which have resisted the most varied and most active treatment. This mode of treatment may appear alarming at first, but when directed by a physician who can watch the progress of the phenomena, regulate the administration of the drug, and combat its effects when excessive, it is not dangerous. Five cases successfully treated by this method were related.

Remedies against Relapse in Intermittent Fever.—In a case of intermittent fever, which yielded readily enough to the usual remedies, but always returned when these were given up, Dr. Kuntzmann of Berlin saw the disease effectually subdued by the exhibition of a table-spoonful of the powdered stem of the pumpkin, in half a table-spoonful of tincture of wormwood. This dose was given every day at the same hour, and when indications of an approaching paroxysm were experienced. Dr. Osann says, that relapses of intermittent fever are best prevented by the use of the Tinct. Absinthii, which he has long been in the habit of prescribing, along with an equal quantity of the Tinct. Cinchon. Compos.—*Lond. and Monthly Jour. Med. Sci.* Nov. 1841, from *Hufeland's Journal*, June 1841.

Treatment of Scrofulous affections by the preparations of Walnut leaves.—M. Negrier of Angiers has used the above remedy in scrofulous children, nine of whom had osseous enlargements with caries; seven ulcerated glands; one several

swollen cervical glands with scrofulous ophthalmia of both eyes. Each patient took daily two or three cups of infusion of bruised walnut leaves sweetened with sugar or honey, and a four-grain pill of the extract of the leaves, or a spoonful of a syrup prepared with eight grains of the same extract to ten drachms of syrup. All the sores were washed with a strong decoction of the leaves, and covered with linen compresses steeped in this decoction, or poultices made with flour and the decoction. Seven of the patients were completely cured after six months of this treatment, and five nearly so: and Mr. Negrier concludes that the walnut leaves are superior to all other antiscrofulous remedies.—*Brit. and For. Med. Rev.* Oct. 1841, from *Bull. Gén. de Thérap.* May 1841.

Prevention of Tubercles.—M. Coster has submitted several animals to various experiments, for the purpose of determining how far the formation of tubercles may be prevented by diet, but principally by the administration of medicines with their food. Some rabbits were fed in the open air, and in the usual manner; another set were shut up in narrow boxes, in a cold, moist place, and deprived of light, air, &c.; they were fed on potatoes, turnips, &c. A third set were placed in the same circumstances as the latter, and nourished in the same way, but they were given every second day some bread containing nine grains of carbonate of iron. The animals were killed after the lapse of five months. The first set were healthy; the second set had tubercles in the lungs or other parts of the body; while the third set remained completely free from any trace of tubercle. M. Coster assures us that he has performed similar experiments on dogs, chickens, &c., and invariably found that the bread containing iron prevented the formation of tubercles.—*Provincial Med. and Surg. Jour.* June 12, 1841, from *Bull. de l'Acad.* No. 13.

The use of Opium in Acute Internal Inflammations. By Robert Christison, M. D., Professor of Materia Medica in the University of Edinburgh.—1. The varieties of internal inflammation which best exemplify this action of opium, when given singly, are the inflammations of the mucous membranes. Of such diseases there are at least four which may often be thus successfully treated without almost any other remedy, namely, coryza, catarrh, influenza, and dysentery.

As to *Coryza*,—most persons on feeling its approach, are content to submit without a struggle to the infliction, and eschewing alike physic and the physician, let their "cold in the head" take its own way. But few would do so, were they aware how easily and how agreeably so tormenting a visitor may for the most part be got rid of. For twenty years I have been accustomed to see it stopped at once by a full opiate given on the first day of its appearance. Let the patient avoid food after dinner, use liquids sparingly, take a full dose of muriate of morphia, or Battley's solution, at bedtime, and breakfast before getting up in the morning,—and he will then commonly find the secretion of the nostrils permanently inspissated, and the complaint either gone entirely, or at any rate no longer a source of particular annoyance.

During the same period I have often seen a common *catarrh without fever* cut short in like manner, if taken on the first, second, or perhaps even the third day. Or more generally it seems to pass at once over the intervening stages to that in which thick sputa are coughed or hawked up without labor, and without irritation in the chest or windpipe. *Febrile catarrh* too may be checked abruptly in the same way, if patient and physician are lucky enough to meet during the first or the second day at farthest. But here probably the next mode of employing opium is fully more successful. I have also repeatedly seen epidemic *influenza* thus checked at the outset,—the local inflammation vanishing, while the strange lassitude, listlessness, and ennui, so characteristic of this disorder, went on as usual for some days during convalescence.

Although an authority so recent and so eminent as Mr. Pereira states, that "in *dysentery*, opium can only be used beneficially in the latter stages, and then with great caution" (*Mat. Med.* 1303), this is another and more formidable malady, which, if I mistake not, may be added to the mucous inflammations capable of being cut short in the early stage by opium. I doubt whether Mr. Pereira's doctrine will be received at all as a general proposition. There is not a better way of treating an ordinary mild dysentery at the commencement, than by the familiar practice of alternate opiates and laxatives. But—what is more connected with the matter now chiefly under review—in severe epidemic dysentery, as it occurs at times in this country, the cure when early begun, may be commonly trusted, if I may rely on my own experience, to opium alone,—not used, however, "with great caution," but given boldly and often.

2. The method of using opium along with ipecacuan as a

sedative anodyne, and sudorific, in the early stage of acute inflammations, is probably applicable to a considerable number of diseases of the kind. Those alone in which I have employed it are common sore throat, catarrh, and acute rheumatism.

From frequent observation, I am inclined to think that there are few cases of *acute cynanche tonsillaris*, which may not be cut short at the outset by this treatment, if they are subjected to it about the close of the first, or at all events before the close of the second day. Of several instances I have met with to this effect, the following are examples. A lady, rather subject to febrile cynanche tonsillaris, was attacked with rigors in the evening, and sore throat during the night. Next day the right tonsil was much enlarged and red, and the posterior palate and velum red and thickened,—the pulse at the same time being one hundred and twenty, and the febrile oppression considerable. In the evening, and therefore within the first twenty-four hours, ten grains of Dover's powder were given at intervals of half an hour, till half a drachm was taken. Perspiration which soon broke out gently, was kept up for fifteen hours by warm drink. In twenty hours after the powders were taken, the pulse was seventy, the pain in swallowing gone, and the swelling and redness insignificant; and on the subsequent morning she was able to leave her bed. This attack happened eight years ago at least, and the disease has not returned since. A lady about thirty-eight, long a martyr to cynanche tonsillaris,—which, in particular for three years before, had returned during the winter, ended in abscess of one or both tonsils, occasioned great and protracted torture, and had left chronic enlargement of these glands,—was attacked in the same way for the fourth time. When I first saw her on the fourth day of the disease, the tonsils, greatly enlarged, especially on the left side, blocked up nearly the whole throat. She spoke with much difficulty, could not swallow without long, painful, and repeated efforts, and breathed noisily and with labor. The pulse was one hundred and sixteen, full and soft. The febrile oppression and anxiety were great. A large blister had been applied to the throat, and purgatives taken freely, but without the slightest benefit. The attack seemed too far advanced to be arrested. But as fluctuation could not be detected in the larger tonsil, Dover's powder was ordered as in the former case. Sweating ensued; ere long, warm drink could be swallowed with no great difficulty: and when the perspiration had thus been kept up for twenty-four hours, the pulse had fallen to sixty-four, the swelling of

both tonsils had materially subsided, and the febrile anxiety had ceased, together with the local uneasiness in a great measure. In two days more there was little complaint left except weakness. In the course of time, the chronic enlargement of the tonsils disappeared; and this lady, like the last, has never had another attack, though five years have elapsed since the one now described. The tendency to cynanche tonsillaris, certainly seems to grow with its repetition in a severe form; and I have met with other instances besides these two, where an abrupt early cure seemed to break both the attack and the liability. The only objection, or rather obstacle, to this plan of cure is, that patients unluckily will seldom put themselves in the way of profiting by it; as the practitioner does not often see febrile sore throat, till the chief question he has to consider under the head of treatment is, whether an abscess in the tonsil is to be opened by nature or the lancet.

I have seen *febrile catarrh* quickly arrested in the same way in persons prone to it. But the circumstances have been so similar to those just described, that a detail of them here seems unnecessary.

Whether *acute rheumatism* may also be treated successfully by the like means, is a matter of doubt. At the period at which medical men generally first come in contact with a case of it, sweating by Dover's powder will not succeed singly. But this is a powerful and speedy measure, if immediately preceded by general blood-letting. Perhaps, then, the present topic would be better deferred, and taken up under the next head. But since it is the action of opium as a sudorific, which seems here the source of its curative influence, while in the third mode of using it for arresting acute inflammatory diseases of this kind of action is not at all essential, the treatment of rheumatism will be more conveniently disposed of in the present place.

There are physicians of undoubted authority, who maintain the acute rheumatism can scarcely be cut short,—that, do what one may, the patient must endure towards three weeks of agony, and three weeks more of stiffness, weakness, and occasional aches. I am convinced, however, that, at least in young adults of sound constitution, a genuine acute attack may often be put an end to in a few days, by first drawing blood very freely to the approach of faintness, and then giving Dover's powder instantly afterwards in the way mentioned above for inflammatory sore throat. I have often observed, that after sweating has been thus brought out and kept steadily up for thirty-six or forty-eight hours, the pulse fell to the natural

standard from nearly double that rate, the white thickly-furred tongue began to clean, the pains and redness gave place to numbness and want of power, recovery went on afterwards swiftly and without interruption, and the patient was able to leave his bed in a week. The chief conditions for success are, that the bowels, previously opened if necessary, shall be let alone till the sweating is well over; that blood be drawn both largely and to approaching syncope; that the powders be given immediately afterwards, before the circulation recovers its state of excitement, which it will otherwise soon do; that the treatment shall be enforced before the close of the fourth day, but earlier if possible; and that the case shall be real acute rheumatism, not one of the sub-acute form, or gouty rheumatism, as it is called. Where for some days previously the local inflammation has been shifting from joint to joint, with irregularly intermitting fever, I have once or twice tried to do without the preliminary blood-letting, but have not succeeded. In one instance, that of a stout young cabinet-maker, copious sweating begun on the third day without blood-letting, and maintained for eight-and-forty hours, was of no avail; but when repeated after one free evacuation of blood, it put an end to the disease abruptly in the way just described.

3. The treatment of acute internal inflammations by opium after blood-letting is now currently used by some practitioners. But since, notwithstanding its efficacy, it is still not generally practised, it seems to need the support of farther testimony than it has received from the few who have taken public notice of it.

This mode of cure consists in withdrawing blood very freely from the arm till faintness approaches, just as in the ordinary way of treating acute inflammations,—and then giving a large opiate, with a view to bring on sleep or the calm reverie which in some people takes the place of sleep. The result is, that, on the patient awaking, the general fever and local inflammation are found to be subdued and broken,—generally at once, but sometimes not till the repetition of the practice in twelve or twenty-four hours.

The conditions for successfully employing opium after blood-letting are nearly the same with those for using Dover's powder in rheumatism. It is essential that the disease to be subdued be in its early stage; that a deep impression be made on it by blood being withdrawn both freely, and to the approach of faintness; and that the opium be given largely and immediately so as to anticipate the renewal of reaction. Sweating sometimes ensues, but it is not at all a necessary condition for

success. The particular preparation of opium to be used, is perhaps of no great consequence. I prefer the solution of muriate of morphia, or, failing that, the sedative solution of Battley. The dose of the former should not be less than forty minims, for an adult male, and for others in proportion; and the dose of Battley's solution, which is certainly not so strong as its maker represents, should not be less than twenty-five or thirty minims. Some conceive this treatment applicable only to inflammation of membranous surfaces, not to that of parenchymatous textures. I do not know any positive facts either on one side or the other of this question; but the statement is doubtful, if it be meant to apply to acute parenchymatous inflammations in their early stage. If pneumonia be regarded as inflammation of a parenchymatous tissue, which, although a common mode of viewing it, is rather incorrect,—then, there can be no doubt that in this particular instance the treatment is most effectual on many occasions.—*Braithwaite's Retrospect Pract. Med. and Surg.* No. 3, from *Edin. Monthly Jour. of Med. Sci.* Feb. 1841.

Antidote to the Salts of Copper. By W. Benoist, Pharmaceutist at Sancoius (Chev).—Liquid albumen is at present almost exclusively adopted as an antidote for the salts of copper, since it possesses the property of decomposing and precipitating almost all the metals, and of forming albuminous compounds with the metallic oxide and the acid. But we are exposed to serious consequences in poisoning, if after having excited and facilitated emesis, we should administer to the patient a large amount of albumen, for the purpose of more certainly neutralizing the poison; what happens? The cupreous precipitate will be dissolved in the excess of albumen. If, on the contrary, too small a quantity of this latter is employed, it will remain dissolved in the excess of solution of copper. To obviate this inconvenience, we have no means, for we do not know what amount of albumen is necessary to neutralize a given quantity of any salt of copper and were this positively known, we could not properly estimate the quantity remaining in the stomach after emesis.

I am about to propose another agent which does not offer the same inconvenience; I mean a solution of carbonate of soda, which forms with the salts of copper precipitates of a green color, insoluble in water, carbonate of copper. It is on account of its insolubility in water, that this carbonate has

no action on the animal economy. I have experimented on animals; some I caused to take acetate of copper, others the sulphate, and to others I gave at the same time both the salt of copper and alkaline solution. In every case the results were satisfactory. I caused a dog of moderate size to take 35 grains of a saturated solution of carbonate of soda. This quantity, more than sufficient to neutralize the poison, did not produce any unpleasant effects. I judge that this salt may be exhibited with impunity, in large amounts, as in the above cited case it only produced some alvine evacuations. The large quantity of carbonic acid which it contains (in 100 parts, carbonic acid 45, soda 31, water 29), causes it to be less caustic than the other alkaline carbonates. I have not yet had occasion to employ this in any case of poisoning in the human subject. I may observe that we ought not to lose sight of the indispensable necessity of exciting vomiting, so as to expel as much as possible the excess of poison.

Before M. Orfila had elevated toxicological science to its present rank, we may call to mind what were the means used to counteract the effects of the preparations of which I have spoken: solutions of sugar; these means being effectual, how can we explain the result? It may be that the carbon of sugar coming in contact with the oxygen of the water and of the oxide of copper, passes to the state of carbonic acid, and then the same reaction is produced as by the means I have recommended.

We have a recent example of the effect of sugar in an observation of M. Lesage, relative to a young man, who, wishing to destroy himself, took the sulphate of copper in a highly sweetened drink, and by accident escaped, the poison being thus neutralized.

From the foregoing, we may conclude that if sugar by one of its elements, may give rise to the formation of carbonic acid, and form insoluble compounds with the salts of copper, we should not be astonished that the bicarbonate of soda should possess this property in a higher degree.

Milk has also been for a long time considered an antidote to the salts of copper; the acid, it is true, separates the curd which precipitates; but it does not really render much service under these circumstances, unless it acts as a soothing application.—*American Journal of Pharm.* from *the Jour de Chimie Med.*

New Process for the Detection of Copper, applicable to Medico-legal Analysis. By M. VERGUIN, of Lyons.—This process was suggested by an observation made some years since in analysing a mineral of copper. By accident, I placed the solution in a platinum capsule, and wishing to estimate the copper in a metallic state, placed therein a plate of iron. While the iron was not in contact with the platinum, no action took place; but the instant it came in contact with this latter, the capsule was covered with a very adherent film of copper, no precipitation taking place on the iron. The adhesion was so strong, that to separate them, nitric acid had to be used. I paid but little attention to this fact, and had almost forgotten it, when it was called to mind by reading the process of Dr. Christison for the detection of mercury; and I sought out a simple process which might be applied to the determination of copper in medico-legal analysis. It is this which is the subject of the present note.

Before entering on the description of my process, I will rapidly examine the different reagents employed—their degree of certainty, and the case in which they will be insufficient. These reagents are ammonia, the yellow cyanide of potassium and iron, and metallic iron.

Ammonia acts by dissolving the oxide of copper, and producing a fine blue color; this color is with difficulty perceived, if—1st, the liquid tested contains a salt of which the base is precipitated by the ammonia, for then it is disguised by the precipitate; 2d, if it is colored by organic matter. It is true we may filter and decolorise by animal charcoal—but when but little of the substance is possessed, it is very important not to lose any by the number of manipulations.

The cyanide of potassium and iron detects minute quantities of copper; for this it is necessary that the liquid should be pure, and especially that it should not contain a trace of iron, for without this it is impossible to distinguish the brown color of the salt of copper, when mixed with the blue of the salt of iron.

Iron acts by decomposing the salt of copper and precipitates the copper in a metallic state, an action well represented by the following formula: (Cu3S) + Fe = Cu + (Fe3S), where we may see that the iron takes the place of the copper, and when the reaction terminates, we have sulphate of iron and metallic copper. But it is necessary that the liquid should be acidulated by a little acid; but if too much be ad-

ded and the copper be in small amount, the iron blackens, and prevents the copper being readily distinguished. Moreover the copper does not adhere, and the least disturbance causes it to separate.

These uncertainties do not exist in the process which I am about to describe, and which is no more than the application of the observation of which I spoke in the commencement.

It is proper that the liquid to be examined, if it be dilute, should be somewhat concentrated, and slightly acidulated with hydrochloric acid; a drop is to be placed on a plate of platinum, and covered with a well polished plate of iron, in such a manner that the iron shall touch both the liquid and platinum. In a few seconds the platinum exhibits a very adherent covering of copper throughout every part touched by the liquid. The explanation of this fact depends entirely upon electro-chemical theory; it results from principles which I shall rapidly enumerate: 1st, when two metals are placed in contact, there is a production of electricity, one of the metals being positively and the other negatively electrified. 2d, when a solution is subjected to the action of the pile, the salt is decomposed, the acid passing to the positive, and the base to the negative pole. There are some salts which are not only thus decomposed into acid and base, but still further the base itself is decomposed into metal and oxygen; in this case the metal only passes to the negative pole, the oxygen appearing at the positive pole.

But, when iron and platinum are placed in contact, there is a developement of electricity, which developement is the more active when a saline solution is present; the iron becomes positive, the platinum negative. The salts of copper have the property of being decomposed not only into acid and base, but into oxygen and metal. Hence the acid being more oxygenated is transferred to the iron or positive pole, while the metal is fixed on the platinum or negative pole.

Such is the process which I offer as most exact, and not liable to the uncertainties of the other methods. It is simple, does not require any manipulation which may not be performed by persons with but a slight chemical knowledge, and I believe will be found useful in medico-legal experiments.—*American Journal of Pharmacy* from *the Jour. de Pharm.*

Statistics of the Lateral Operation of Lithotomy at Naples. By M. S. DE RENZI.—From the year 1821 to 1828, inclusive, 596 cases of stone were operated on, 579 of whom were males, and only 17 females. Of this number 503

recovered, and 91 died; 306 of these cases occurred in persons under 15 years of age; 231 in adults, and 59 in aged individuals. As a means of comparing the mortality from this operation, the results of the operations of lithotomy for the year 1839 are given. During this year 47 were operated on, 46 of whom were men, and 1 woman. Of these 38 recovered, and 9 died. Of this number 15 were below 15 years of age: 31 were adults, and 1 was aged.—*Ed. Med. and Surg. Journ.*, from *Il Filiatre Sebezio Giornale.*

Reduction of a Strangulated Hernia, apparently effected by Acupuncture. By Dr. Daser.—A man, 50 years of age, was seized with strangulated hernia, with vomiting of stercoral matter and all the other usual symptoms. The taxis having failed, Dr. Daser, before having recourse to the operation for strangulated hernia, with the view of trying the effect of acupuncture, for the purpose of evacuting the gaseous contents of the strangulated portion of the intestine, made two punctures in it with a long fine needle. No gaseous matters apparently escaped, but the patient complained of acute pain, and loud gurgling sounds were heard in the abdomen, immediately after which the hernia was spontaneously reduced. Dr. Daser attributed this fortunate occurrence to the prick of the needle having excited contractibility of the intestine, causing it to contract on its contents, expel the gaseous matter into the abdominal portions of the gut, and thus facilitate its reduction.—*Ibid*, from *Journal fur Chirurgie und Augenheilkunke*, 1840.

Hæmorrhage after Lithotomy stopped by Creosote. By Dr. Daser.—In a case of lithotomy it was found impossible to arrest the hæmorrhage by any of the usual means, and no particular vessel could be discovered from which the blood might flow. The patient was at last reduced to the lowest ebb from the continued loss, and had already lost consciousness, when a sponge dipped in pure creosote was introduced into the wound, and pressed against the bleeding parts for an instant or two. The hæmorrhage was immediately arrested. No particular pain was experienced, no unpleasant symptoms followed: thin eschars were thrown off, and the patient recovered.—*Ibid.*, from *Journ. fur Chir. und Aug.*, 1840.

The conditions which favour an excessive size of the Fœtus.—Professor OSIANDER regards a too exclusive use for food, of articles composed of fecula, especially rye-bread, as contributing to produce a sort of fœtal hypertrophy; and he recommends to pregnant women who are obliged to live on such a diet, to abstain at regular times from food, and to take occasionally a saline purgative. The excessive developement of the fœtus is one of the causes of difficult labor, and the Professor relates five cases of this nature.—*American Journal of the Med. Sciences*, from *Zeitschrift fur gesammte Med.*

Medical Quarrels.—On the old principle that "misery loves company," our readers may not be sorry to hear that all the medical squabbling is not confined to our city, nor even to our side of the Atlantic. Paris is at present the seat of a very pretty quarrel. It seems, M. Gerdy clinical Professor of the Faculty of Medicine, in a discourse lately pronounced by him at the opening of the session, has given mortal offence to some of the Parisians. Gerdy took for his theme, the life and character of Sanson, the lately deceased Professor of clinical surgery, and it cannot be denied that he did full and noble justice to his late colleague. The severe virtues of Sanson, his high and inflexible principle, his generous disinterestedness, his perfect honesty, are all well appreciated. This however need not have given offence and perhaps did not; but M. Gerdy went further, he exposed the ingratitude of Dupuytren, and his cold-hearted selfishness towards his pupil and his very devoted friend. Dupuytren it seems used Sanson in every way without scruple, availed himself of his literary talents, and yet never would forward his views, but rather used his influence to keep down Sanson, to keep him in the background. All this is brought out without ceremony by M. Gerdy—he also lets us into a piece of literary history, which is curious and amusing, viz: that Sanson *wrote part if not all of Dupuytren's Thesis* for the Concours at which he gained the Chair of Surgery. Dupuytren was, it appears, a slow and difficult writer, eternally revising, correcting and altering his manuscript—never satisfied—hence it happened that as the time for presenting the thesis approached, Dupuytren could not finish his, and actually wrote a letter to the Faculty, announcing his withdrawing from the Concours. Duprey, one of his friends, saw the letter and withdrew it. He afterwards persuaded Dupuytren to go forward, and then Sanson came

forward in the fullness of his devotion to his master, finished the thesis, and thus opened to Dupuytren the career in which he gained so much fame and so plentiful a fortune. It is to this man who so signally *saved him* in his hour of need, that Dupuytren is said by M. Gerdy to have been ungrateful.

This accusation against their surgical idol, has united the Parisian Journals in a most furious tirade against Gerdy. He is denounced without stint or measure, and even without decency—meanwhile the facts will all probably come out, and we fear that it will be manifest that the great Parisian Surgeon was as deficient in moral qualities, as he undoubtedly was eminent for intellectual powers. Meanwhile, whatever may happen to the master, the pupil is sure to occupy a most enviable position. All parties unite in doing honor to Sanson, and he, who lived in poverty and suffering—who starved while Dupuytren was rioting in wealth, is now honored, beloved, venerated—and that, not because he was learned, or wise, or bold, or daring, but because he was good, because he was honest, because during his surgical life, he was ever under the influence of high moral principle, because no temptation ever induced him to swerve from the strait and narrow, and as he found it, the thorny path of strict professional honor and honesty.

Such was Sanson; he is dead—but *his* memory lives in green and beautiful freshness. Dupuytren too is dead—*his* memory lives, but alas! his friends are already reduced to the sad necessity of attempting to drown by clamor the voice of Gerdy, who has dared to tell that truth which none of them dare deny. There is a lesson in all this which ought not to be lost upon us—few can hope to rival the splendid achievements of Dupuytren, but all may practice the virtues of Sanson.—*N. Y. Med. Gaz.*

Researches into the real constitution of the Atmosphere. By MM. Dumas and Boussaingault.—It is generally admitted that the air is composed of a mixture of oxygen and nitrogen, and its invariableness is explained by supposing that the green parts of plants under the influence of solar light decompose all the carbonic acid developed in the respiration of animals, and the putrefaction of organized bodies. Some, however, regard the air as being not a mixture, but a chemical compound of 20 of oxygen and 80 of nitrogen (Prout, Doberein-er, Thomson, &c). Others, and these the majority, consider

it as a mixture of 21 of oxygen and 79 of nitrogen; and, lastly, in the opinion of some (Dalton, Babinet), the composition of the air varies according to the height in the atmosphere.

The plan employed by the authors in submitting these questions to a fresh examination is distinguished from others in that they estimate the weight instead of the measure of the gases, and thus analyze the air by weighing successively the oxygen and the nitrogen which it contains. We cannot follow them into all the details of their experiments, which, by successive corrections, were rendered more and more exact: we can only point out the results.

They fix the density of oxygen at 1.1057, and that of nitrogen at 0.972; numbers a little different from those given by other chemists. They demonstrate that the relation of the volume of the oxygen to that of the nitrogen in the air is not expressed by simple numbers; and that the air cannot be regarded as a chemical compound of 20 volumes of oxygen and 80 of nitrogen. They admit, as *a sufficient approximation*, that the atmosphere is composed, by volume, of 20.8 of oxygen, and 79.2 of nitrogen. They presume that the mixture is uniform in all times, in every latitude, and at every height. "If the atmospheric air," they add, "is a reservoir of oxygen for the use of animals, and a reservoir of carbonic acid for the use of plants, it is so considerable a store that the consumption, supposing it not to be compensated, would remain almost insensible after a long series of years." They have calculated that supposing each man to consume a kilogramme of oxygen per day, and that the oxygen disengaged by plants did no more than compensate for the other causes of its absorption, the whole human race, and three times their number, would not consume, in a century, the eight-thousandth part of the oxygen which nature has placed in the respirable air.—*Lond. Med. Gaz.*, Oct. 15, 1841, from *L'Examinateur Medical*, Aout 20, 1841.

On the Influence of the last Inundation on the health of the Population of Lyons.—It was a matter of much interest to determine what were the effects on the health of its inhabitants of the inundation, which, in so short a time, on two different occasions, covered a part of the town of Lyons. If mere variations of temperature and atmospheric moisture do exercise on the health only a part of the fatal effects attributed to

them, it is certainly, in a case like that of Lyons, that such effects ought to be easily appreciable. In no inundation of that town had the waters ever before risen to such a height: in none was their stay so long, or their retreat so slow. They covered the town and the country round over an area of several leagues; they inundated the quays, the streets, the places of public resort, the cellars, the houses; and they polluted the waters of the wells, the pumps, and springs, with heterogeneous substances that filtered into them from the sewers and the privies. After their retreat a stinking sediment covered the majority of the streets, and a thick viscid mould lined the inner walls of the houses; the atmosphere was charged with a nauseous dampness; the population was distressed; everything was dreary; and yet the inundation of 1840 had but a just perceptible influence upon the public health. From the month of November in that year, to the present time (July, 1841), the hospitals have not been more than usually encumbered: the number of patients has been but little increased, and, though the mortality has been distinctly greater, yet this cannot be attributed to any disease in particular produced by the inundation.

In the days immediately following this scourge, a considerable number of cases of obstinate diarrhœa, and some of dysentery attributed to drinking the unwholesome water, were observed. Some typhoid fevers also appeared; and, if the state of the atmosphere had any influence upon them, it was only by prolonging their continuance, and giving them a more marked adynamic character. Still they were not more fatal than they usually are, though it was necessary to modify the ordinary treatment; blood-lettings at the outset were less employed, and tonics were sooner and longer administered.

At a later period numerous and obstinate rheumatisms were observed, but they rarely had an acute character, except in those who had worked long in the water: in general the moisture of the atmosphere rather revived old pains than generated new ones. The diseases which reigned most generally, were catarrhs and catarrhal fevers, which, however, are habitually prevalent at Lyons at this season; but the inundation remarkably increased their number and modified their character; some affecting the lungs, had an alarming intensity and obstinacy, but on the whole they destroyed but few. An epidemic disease, of which both the etymology and etiology are still obscure, reigned at this time at the garrison, but it could not be, in any measure, traced to the effects of the inundation.

Notwithstanding the absence of any special affection, however, the mortality was more considerable than during the preceding years. Thus, in the last two months of 1840, the town and hospitals together had 1118 deaths; while in the same two months of 1839, there were but 787, and of 1838 only 750. But this difference is easily explained by the pernicious influence such a scourge would have on aged, weak, and diseased people, and especially on such as were already affected with chronic diseases.

The author attributes the good state of health of the town and its environs, during the continuance of the scourge, to the prompt and energetic measures which were taken by the local administration, and to the north wind, which continued for twenty days and greatly aided the sanitary measures that were employed.—*Lond. Med. Gaz. November* 5, 1841, from *Journal de Medecine de Lyons*, Juillet, 1841.

Treatment of Asphyxia from drowning.—As most practitioners in the city or country are occasionally called upon to attempt the resuscitation of persons apparently drowned, yet not so frequently as to bear in mind at all times the differences of opinion among the more experienced, as to the artificial respiration, and the time at which it becomes useless, we extract the following remarks from a debate on the subject before the Westminster Medical Society, in October last, as published in the London Lancet for October 30th.

Mr. Forbes Winslow led off this portion of the debate, which was collateral with a discussion on the various forms of asphyxia in new-born infants, eliciting nothing new, as far as we can judge from the report.

"Mr. Winslow was of opinion that inflation by air breathed from the mouth of the practitioner was the best plan of carrying on artificial respiration. Experiments had shown that the air so used was but little altered in quality, and contained only one-hundredth part less of oxygen than it did previous to its being respired. With respect to the recovery from asphyxia by drowning, Dr. A. T. Thomson had related a case in which a person who had been twenty minutes under water was restored; while other cases were recorded, in which three minutes' immersion was sufficient to entirely destroy life, and frustrate all efforts for its revival. How did we account for this discrepancy? The most experienced pearl-divers could not remain under water more than five minutes. He

(Mr. Winslow) thought that when persons fainted immediately on submersion, vitality was longer retained in consequence of there being less demand upon the system than when struggles were made by the sufferer.

"Dr. Chowne would look with suspicion on any case of recovery from asphyxia by drowning, in which it was stated that the person had been under water for more than six or seven minutes. He believed, indeed, that there was no well-authenticated case of recovery in which the submersion had exceeded six minutes."

"Mr. Wooley thought Mr Read's instrument would be of great service in asphyxia, whether occurring in adults or infants. He had been in the habit of using the means recommended by the Humane Society to keep up artificial respiration, for some years. The instruments used were the bellows and the trachea-tube. The use of the bellows might be productive of much injury; and it was not always an easy matter, even to those who knew the anatomy of the parts well, to insert the trachea-tube into the proper orifice. Mr. Read's instrument did away with these objections, and he considered it an admirable means of keeping up artificial respiration." "In asphyxia from drowning, however, he had seldom found much benefit from attempts to keep up artificial respiration. When a body was brought to the receiving-house, it was immediately placed in a bath at 100°. Respiration most frequently occurred spontaneously whilst the person was in the bath, and in some cases it took place before the patient had arrived at the station. If, after the body had been placed in the bath, there was no appearance of vitality, he had generally found the employment of artificial respiration of no benefit. Respiration, when first returning, was generally interrupted and spasmodic; it then became regular, and frequently no ill consequences followed. There was, however, in many cases, much mischief supervening in the form of congestion of the brain and thoracic viscera. In some cases of this kind, the convulsions were so great, that the patient was held with difficulty whilst blood was abstracted. In these cases the only instrument he had found of service was the lancet; and it was often necessary to take away a considerable quantity of blood."

Mr. Wooley then read a case illustrating the length of time during which life may be preserved. It was first published in the report of the Royal Humane Society in 1840, but as the report is in few hands in America, we extract from it every thing essential.

"At about half-past ten in the morning, a police-constable on duty, and the gate-keeper, saw a man, a little more than half way over the bridge, place his hat upon the pavement, mount the parapet, and leap into the water. The policeman ran to the spot where the occurrence had taken place, and the gate-keeper to his lodge, in which was one of the society's speaking-trumpets, to use it in calling for a boat; but before doing so, he took out his watch, and carefully observed the time. When he saw the boat coming, he ran and joined the policeman and others who were observing the man in the water. From the time of his jumping in he had remained under the surface, supported in a perpendicular position by the flaps of his coat, which lay *upon* the surface, a small portion of the scalp being out of the water. His face was never above the surface, and a great number of small air-bubbles kept coming up from the commencement. He moved very little—at length a large bubble came up, and he immediately began to sink deeper; the bystanders now exclaimed that it was all over with him. At this moment the boat reached the spot, when he had sunk so low that his coat tails were but just within the reach of the arm of the man employed by the institution, who, however, got hold of them, and pulled him into the boat. The gate-keeper then looked at his watch, and found that exactly *five minutes* had elapsed since he first noted the time. The man was apparently dead; but just after he was laid down in the boat a low moan was heard, another midway between the bridge and the receiving-house, and a third when he was on the stretcher being carried into the house. There was, however, no *appearance* of breathing or of life when he was put into the warm bath, which was ready upon his arrival; but, almost immediately after, convulsive and irregular breathing commenced—he breathed irregularly and moaned occasionally for about fifteen minutes. In five minutes more I saw him; his breathing was then quite natural, his pulse regular and good; he had no pain in the head or chest, nor any where else, and felt pretty well; his face, however, was very pallid, and his eyes suffused: he looked like a man who had suffered from habitual intemperance, which I found was the case. I now had him taken from the bath, and put upon the warm bed, apparently free from cerebral or *thoracic* congestion, and only wanting strength to enable him to go home. As the ward was too hot and close, I desired that a little air might be let in, and left the room for a short time. On my return, I found that my order had been misunderstood, and all the windows had been opened. I was absent only a very few

minutes in the adjoining room. The effect of this admission of cold air was to produce oppressed breathing and much cough, and to require treatment and watching, which detained me six hours at the receiving-house; they would not take him into St. George's Hospital, and, till the expiration of that time, I did not think it safe to send him home. This shows the importance of warm air in resuscitation."—*Philad. Examiner.*

Connection between abundance of food and mortality. By M. Melier.—In this memoir, which was read at the Academy of Medicine of Paris on the 7th of September, the author established, by numerous documents drawn from the histories of various countries, that the number of deaths always correspond to the price of food. "Wherever there's a loaf added there's a man born," said an economist; and nothing is more true than this metaphorical expression. If we represent the variations of the general mortality and those of the price of bread at different times, by two curved lines which rise and fall with all the fluctuations of these particulars, we shall find all their curvatures exactly, and with the most perfect regularity, corresponding. The constant increase of the population of France for a certain number of years, is easily explained by the progress of agriculture, the modifications which the laws relating to corn have undergone, and especially by the introduction of potatoes. The influence of the dearness of food, however, is observed more distinctly in the year next following than in any in which it has occurred.—*London Med. Gaz.*, from *Gaz. Med.*, September 10, 1841.

Copy Right in Lectures.—The question as to what right a lecturer has in his oral lectures—and whether he can prevent their being published by any one who may attend them and take notes for the purpose, has recently been brought before the courts of our city. Dr. Mott applied to the court of chancery for an injunction restraining Dr. James A. Houston from publishing his, Dr. Mott's Lectures, in the Lancet. This injunction was sought on two grounds, first, that Dr. Mott had hired Dr. H. to take notes of the lectures, and paid him a valuable consideration therefor, and that part of the bargain was, that no public use should be made of the notes. The second ground was the general principle, that a public lectu-

rer has a right in his lectures, and that it is a violation of that right for any person to publish the lecture, or notes of it; that notes can only be taken for the individual benefit of the student, and not to be published for gain. These were the grounds on which Dr. Mott sought to prevent the publication of his lectures in the Lancet. In support of the application, David Graham, Esq., appeared before the assistant Vice-Chancellor, Murray Hoffman, Esq., and Dr. Houston was defended by Mr. Sherwood. The case was fully argued on both sides, and on Wednesday last the opinion of the court was given. The Assistant Vice-Chancellor held, that the State Courts had no jurisdiction in the matter, it belonging exclusively to the U. S. Courts; upon that ground the injunction was, substantially, dissolved. The learned judge however expressed *a very decided opinion, that Dr. Mott could restrain the Editor of the Lancet from publishing his lectures*, on the ground of the bargain alleged by Dr. M. to have been made, whereby Dr. H. expressly stipulated that he would make no such use of the notes, which Dr. M. employed him to take. As to the general question, whether a public lecturer can restrain a student or other person from publishing notes he may take of the lecture, the Court had great doubts. The decision of Lord Eldon in the case of Mr. Abernethy, versus the Lancet,* could not be considered as settling the law upon this matter, although it is quoted with approbation by Judge Story and Chancellor Kent.†—*N. Y. Med. Gaz.*

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* It seems to be a very general opinion among medical men, that in this case the English Court of Chancery decided against the right of a lecturer to prevent the republication of his lectures; this is a mistake, the decision was exactly the reverse. Lord Eldon gave a long and very able opinion in favor of Mr. Abernethy, in which he sustained fully the ground taken by the plaintiff, viz.; "that a lecturer had a right in his lectures, which none of his hearers could invade, by publishing notes of his lectures." At a subsequent stage of the proceedings the contest was abandoned by Mr. Abernethy, and consequently the injunction was dissolved by default, no one appearing to support it. As the law now stands in England, the right in question is possessed by every public lecturer; the expediency of exercising this right is of course another and a very different question.

† We understand that Dr. M. has appealed from this decision, and proposes to pursue the matter in the higher courts.

THE WESTERN JOURNAL.

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THE TREMBLES AND MILK SICKNESS.

Some ten or twelve months since we published in the Journal a memoir on these maladies, as they have prevailed *between the Little Miami and Sciota rivers, in the State of Ohio.* In this memoir we included nothing previously published by any person concerning those diseases in that quarter, much less any where else, nor did we introduce any of our own unpublished remarks made in other parts of the West. It was in fact a monograph of those diseases, as they occur in *that* particular district; and all the conclusions at which we arrived, were from the facts collected while exploring it. In no case whatever, did we inquire whether these conclusions were either confirmed or invalidated, by facts observed beyond the narrow limits prescribed in our paper. Of the conclusions to which we refer, the most important were, that certain localities within the district generated the Trembles, while others are exempt; that in the former there are numerous scions from the roots of the climbing *Rhus radicans* of Linnæus, and are known by the people under the name of poison oak; that the destruction of the natural vegetation of these spots puts a

stop to the further progress of the disease; and that the facts collected show that this species of *rhus may* be, but do not prove that it *is*, the cause of Trembles.

Now, it is really amusing to see what a stir these and some other incontrovertible *facts*, in the same memoir, have raised among the milk-sick-literati of the West, each one of whom is nursing a special hypothesis, for which he is not only willing but desirous of fighting.

Some of them have merely heard of our memoir—others have seen, and misunderstood its plainest statements—others have wilfully misquoted and misstated its facts! Not one has visited the district, and ascertained that the memoir embraces a single false observation! Some have discovered that we are visionary and speculative, although we consulted more than a hundred persons, before admitting the fact, that calves experience the Trembles from the milk of their dams, and that dogs die from eating the flesh of animals dead from Trembles. We would like to know which of our sick-stomach-literateurs, has devoted a fortnight of diligent personal inquiry to the ascertainment of those facts? Our inquiries about the *rhus radicans* were conducted with equal patience and caution; and at the close we asked whether all the facts collected "*prove*" it to be the cause of Trembles; to which the answer given in the next line is, "*certainly not.*" Notwithstanding all this, we are charged with having declared this plant the cause! and then abused for loose observation, wild theorising, ignorance and falsehood! The worthies who agree in all this, disagree, however, in every thing else, for while each is showing off his own nursling, he is running down that of his rival. We congratulate the gentlemen on having such a fountain of excitement, now when the temperance reform has nearly closed up the alcoholic; and are not quite so misanthropic as to prevent them from misrepresenting and abusing us, whenever they can find enjoyment in it. All we ask, is the privilege of telling the public, now and then, that not being able to controvert what we have written, they have undertaken to controvert that which we have not written. D.

ARSENIC AND THE TREMBLES.

We understand from authority on which we can rely, that Dr. Seaton, a new champion of the old hypothesis that arsenic is the cause of Trembles, has actually succeeded in killing a cow with that poison!

We trust that no one will hereafter doubt its being the cause of that disease. To the incredulous we present the following syllogism, which may help them out: Cattle die of Trembles—a cow died from taking arsenic, *ergo*, the Trembles are produced by arsenic: *quod erat demonstrandum*. D.

KENTUCKY SCHOOL FOR THE INSTRUCTION OF THE BLIND.

Last winter, when delivering some popular lectures on Physiology, in the Hall of the Medical Institute, we took occasion to explain the different modes of teaching the blind; and urged the necessity for a school in this State. Mr. Bullock, a representative from this city, soon after introduced into the Legislature, a bill for that purpose, which passed one branch, but was laid over in the other. This winter, Mr. Bullock again brought up the subject, and at the request of Mr. Patton of this city, Dr. Howe of Boston came out with one of his pupils, and Mr. Chaplin, the Superintendent of the Ohio School, came from Columbus, with three of the pupils of that institution. At Frankfort they made two exhibitions, with the effect of securing the passage of the bill, with an appropriation conditioned on the contribution of a sum of money by the people of Louisville. In this city, they made several exhibitions, and gave an impulse to the work of contribution. Thus, there is much probability, that within the present year, this interesting institution will be opened. We hope, however, that our medical friends will not relax in their efforts to prevent blindness, in consequence of reading this paragraph; and that none of them will advise parents to procure glass eyes for their children, instead of sending them here when the school shall be opened.

D.

OHIO LUNATIC ASYLUM.

In the elaborate and well digested article of Dr. Jarvis, (No. 24 of this Journal) ample justice is done to the *Ohio Lunatic Asylum*, by an abstract from its annual reports for 1839 and '40. Since the publication of that review in December, we have received the Report for 1841, which presents evidence of continued success in its management; but, unfortunately, its benefits can not be extended to all the insane of the State; for, extensive as the edifice is, it appears that from

the opening of the institution, Nov. 1838, to the date of this Report, three years, no less than 167 applications, formal and informal, from that State, had been refused, to say nothing of 1 from Wiskonsan, 1 from Alabama, 2 from Iowa, 2 from New York, 2 from Virginia, 4 from Kentucky, 7 from Illinois, 9 from Pennsylvania, and 13 from Indiana, equal to 41; indicating a want of such establishments in the States surrounding Ohio. Such of our brethren, in those States, as may wish to place their patients in the Ohio Asylum, will perceive, then, that it will be in vain to apply. It will be for them to exert their influence at home for the establishment of new asylums. If Indiana would found one, it might, for several years, answer for herself and her younger sister Illinois. We commend it to our readers in that State, as eminently worthy of their utmost efforts.

D.

KENTUCKY LUNATIC ASYLUM.

In the Review to which we have referred in the preceding article, Dr. Jarvis showed, from the reports sent out by most of the establishments of Europe and America, that the Kentucky Asylum has effected the smallest proportional number of cures, of the whole. This fact, together with his judicious remarks on its causes, and his suggestions for their removal, led the General Assembly to institute a full and searching inquiry into its condition, the results of which are now before us, in the Report of a Committee of its members. Most judiciously the committee recommended an appropriation for the purchase of a farm contiguous to the Asylum, on the necessity for which, and for some other improvementt, they speak in the following language.

"It is believed, that with the addition of the one hundred and seven acres, besides the incalculable advantage to the lunatics themselves, under judicious and proper management, the institution, by their labor, might be abundantly supplied with vegetables, and all the other productions of the soil.

When it is recollected that we are an agricultural people, and that a very large proportion of those who are found in the Hospital are accustomed to agricultural pursuits, and that, therefore, the cultivation of the soil will be not only useful, but agreeable to them, the calculation cannot be deemed extravagant. The Committee is fully sustained in this assumption, by the experience of the Worcester Asylum, in Massachusetts, where the lunatics have not only raised abundantly for the establishment, but have produced a surplus for sale. It is not unusual,

under the judicious and enlightened administration of that institution, "to see two men ploughing in the field, quiet and attentive to their work, and performing it well; both insane, both having committed homicide, and had therefore been confined many years in prison." The pleasing spectacle is exhibited of cheerfulness and contentment. "Those who before, were a pain to themselves, and a torment to their friends, and dangerous to the public, are now mostly quiet, comfortable, and even happy and useful. The sting of their mental death is taken away."

The Committee does not rest the claims of the Asylum to this additional appropriation, on these grounds alone. They are of opinion that it is the *duty* of the Legislature to afford all the means in its power to carry out the great objects of the institution, viz: the *restoration of the insane*, and the amelioration of their unhappy lot. To imprison them, to impose upon them the unnecessary restraints of confinement, to deny to them the common use of their own limbs, to shut them out from the light of heaven, and the enjoyment of the bounties of Providence, is at war with the spirit of the age, and in opposition to the lights of experience and the discoveries of science. They should be afforded the means of every enjoyment, not inconsistent with their wretched condition.

One of the advantages which the Committee proposes to accomplish by this purchase, is, that a residence will be secured, in all respects convenient and suitable, for a Resident Physician. They do not recommend the immediate employment of such an one, but they do not hesitate to express the opinion, that if the necessary arrangements could be made, that the true interests of the institution require it. That every successful Asylum for the insane, of which the Committee has any knowledge, is afforded the services of an enlightened Physician, whose time is exclusively devoted to the investigation and treatment of mental disease, is a fact entitled to the most respectful consideration.

It is not only necessary that the Physician should understand the physical maladies with which the patient is afflicted, but he should be acquainted with his character and the causes of his mental derangement, and by a close and accurate observation, determine upon the best means for his relief. He should be enabled by his manners and character to acquire a mastery over the intellect of his patient, and to conduct it in the proper channel. It should be his province to direct the whole machinery of the institution, with this object kept steadily in view. In a word, all the means employed, not excepting the government and discipline of the establisment, should be under his immediate control. Hence the necessity of a Resident Physician, and providing a suitable and convenient place for his accommodation."

We concur in every word of this extract, and when the Report reached us, fondly hoped, that the Legislature would not adjourn without carrying out the recommendations of the Committee. That hope

has, however, been disappointed; and we now call on the physicians of the State to step forth, and seek so to enlighten and animate the members of the next General Assembly, as to bring about the important ameliorations, demanded equally by the interests of humanity, and the dignity of the State. D.

OHIO SCHOOL FOR THE BLIND.

We have just received the Fifth Annual Report of this institution, and are happy to announce its increasing prosperity. Opening as late as the 4th of July, 1835, it has, already, 50 pupils, of which nearly all are the children of poor parents. But this number embraces a fraction only of those who from that State alone, to say nothing of the other States of the West, ought to be at this moment enjoying the advantages of such an institution. The neglect of parents, especially those of the country, in regard to the early treatment of the inflammations of their children's eyes, is truly lamentable; and the inefficiency of our plans of treatment, is too often quite as apparent. Hence the majority of the blind in the West, become so from diseases, which met in the outset with appropriate remedies, might be subdued. The number of this unfortunate class is annually increasing in all the Western States, none of which, except Ohio, has yet established a school. It is proper, then, that physicians should know, that many additional pupils can be received into the establishment at Columbus, and that the children of other States are not excluded. All the elementary branches of a common education are taught in the school; and also a number of mechanical arts. The expense of a pupil is $100 *per annum*, exclusive of clothing and some contingencies. The Report of Mr. Chapin, the excellent Superintendent, from which we have collected these facts, is drawn up with perspicuity; and has appended to it a number of interesting letters and other compositions, written by the scholars of the institution, which evince a progress as rapid as that made by children who have not lost their sight.

D.